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Cold War Justice: The Supreme Court and the Rosenbergs

MICHAEL E. PARRISH

ON JUNE 19, 1953, THE BEGINNING OF THAT "QUEER, SULTRY SUMMER" DESCRIBED by Sylvia Plath's heroine in *The Bell Jar*, the United States executed Julius and Ethel Rosenberg for conspiring to commit espionage on behalf of the Soviet Union. Nearly a quarter century has not quieted the controversy that surrounded the trial and sentence of the only persons ever put to death by a civil court in America for such a crime. "The Rosenberg case won't go away," Ted Morgan wrote recently. "It lingers, like the smell left in a room after a corpse has been removed. . . . I cannot think of another case in the annals of twentieth-century American justice that has received as much sustained attention over so long a period. The Rosenbergs have become the most internationally celebrated martyrs since Captain Dreyfus."¹

Recent events—including television specials, the popularity of E. L. Doctorow's *The Book of Daniel*, the re-emergence of the Rosenbergs' two sons, and the opening of previously classified documents from the files of the Federal Bureau of Investigation—have stimulated fresh interest in the case among those born or raised after 1953 and revived old passions in those who lived through the era; but no one yet has resolved what are surely the principal issues: did the Rosenbergs conspire to give national defense secrets to the Russians? And, regardless of their guilt or innocence, did they nonetheless receive the full measure of American justice? The answers to these questions in the 1960s, as Allen Weinstein has noted, tended to be influenced by one's perceptions and attitudes respecting the Cold War, Stalinism, and the McCarthy period generally. By the 1970s both the questions asked and the answers given have been increasingly influenced by revelations surrounding Watergate and the lawless behavior of the FBI during J. Edgar Hoover's long directorship.²

I wish to thank the National Endowment for the Humanities and the Committee on Research of the Academic Senate at the University of California, San Diego, for their financial support that made the research and preparation of this article possible. My colleagues, Harry Scheiber and Earl Pomeroy, offered intellectual encouragement and spirited criticism, for which I am very grateful.

¹ Sylvia Plath, *The Bell Jar*, (Bantam edition, New York, 1973), 1; Ted Morgan, "The Rosenberg Jury," *Esquire*, May 1975, p. 105.

² Allen Weinstein, "The Symbolism of Subversion: Notes on Some Cold War Icons," *Journal of American Studies*, 6 (1972): 165; and Allen Weinstein, "The Hiss and Rosenberg Files," *The New Republic*, February 14, 1976, pp. 16–17, 20–21.

On the one hand, there are those who believe that the Rosenbergs were innocent of spying, the hapless victims of anti-Communist hysteria, lying relatives, a vindictive trial judge, and perhaps a conspiracy manufactured by government officials anxious to soothe the public's fears of domestic subversion and the Russian atomic bomb. The Rosenbergs' defenders point out that their arrest and trial took place in 1950–51, one of the darkest periods in the Cold War, at a time when the political fortunes of the Truman administration reached their nadir. Mao Tse-tung stood victorious in China, and the Soviets had broken the United States' atomic monopoly. In addition to these foreign calamities, 1950 brought the conviction of former New Dealer Alger Hiss for perjury, the confession of Klaus Fuchs that he had passed secrets to the Russians, and the intensification of Republican attacks on Franklin Roosevelt, Harry Truman, and the entire Democratic party for betraying the nation's interests to the Communists. Congressman Richard Nixon of California warned the country against "traitors in the high councils of our own government [who] have made sure that the deck is stacked on the Soviet side of the diplomatic tables." The Truman administration, hoping to blunt the impact of right-wing charges, escalated its own anti-Communist crusades. It indicted leaders of the Communist party for violating the Smith Act, pushed the development of the hydrogen bomb, responded unilaterally to North Korea's attack against Syngman Rhee's tottering regime to the south, and prosecuted America's own "atom spies," including the Rosenbergs.³

In addition to the claims made by the Rosenbergs' sons, Michael and Robert Meeropol, the most forceful defense of the couple has been offered by Walter and Miriam Schneir.⁴ Their arguments have persuaded some who once judged the Rosenbergs guilty, including Father Charles Rice, former president of the bitterly anti-Communist Association of Catholic Trade Unionists. He wrote recently in the *Pittsburgh Catholic*, "We were nearly all naive in that day and for years to come. Even the defense attorney trusted the FBI and the prosecutor, and allowed tainted evidence into the record. . . . The Rosenbergs were Communists but probably they were innocent of spying. . . . We were looking for scapegoats. . . . The Korean fracas was going on, the prosecution blamed the Rosenbergs for it, and fools like yours truly believed that."⁵

On the other hand, many remain convinced of the Rosenbergs' treachery and find nothing to question in either the trial or the subsequent legal efforts to overturn the convictions. In his best-selling recapitulation of the case, based largely upon the official trial record, Louis Nizer, for example, deplored the death sentences, but found the evidence against the pair to be overwhelming.

³ Eric F. Goldman, *The Crucial Decade—And After* (New York, 1960), 91–201.

⁴ Robert Meeropol and Michael Meeropol, *We Are Your Sons: The Legacy of Ethel and Julius Rosenberg* (Boston, 1975), especially 347–88; and Walter Schneir and Miriam Schneir, *Invitation to an Inquest: Reopening the Rosenberg "Atom Spy" Case*, (Penguin edition, Baltimore, 1973), 261–443. Other defenses of the Rosenbergs include William Reuben's *The Atom Spy Hoax* (New York, 1954) and John Wexley's *The Judgment of Julius and Ethel Rosenberg* (New York, 1955).

⁵ Quoted in *Commonweal*, December 28, 1973, p. 330.

He noted, moreover, that, of the 112 judges who reviewed the various applications and appeals in the case, only 16 dissented. Although he refused to confront critics of the case directly, Nizer concurred with those contemporaries who argued that "the Rosenbergs received every protection of the democratic process of justice: jury trial, legal counsel, right of defense and of appeal. The United States gave them twenty-seven months of legal review. Their appeals and proceedings exemplify the presumption of regularity and due process attending judgments of conviction. More than two years lapsed before they had exhausted their legal recourses, which is a tribute to the thorough legal processes afforded by American jurisprudence."⁶

The Rosenbergs attracted the attention of ideologues, amateur detectives, and students of the Red Scare. But, although both their proponents and their opponents have long argued the importance of the case to American justice (as either one of the worst or one of the best examples thereof), surprisingly little has been written about the case from the perspective of American legal institutions. Few have analyzed the constitutional and statutory issues raised by the litigation or examined the bitter divisions these issues aroused within the Supreme Court of the United States.⁷ Since 1954 only a half dozen articles about the case have appeared in American law journals; most historical surveys of the Supreme Court do not treat the case in detail and some fail to mention it at all.⁸ The Schneirs' account of the Rosenbergs' postconviction efforts to secure a new trial remains the most accurate version of these tangled events, yet that analysis is both incomplete and inaccurate with respect to the conflicts within the Supreme Court.⁹

But these legal issues presented by the Rosenberg case merit examination and how they were resolved by the nation's courts needs to be assessed. Now, in addition to letters and memoranda from the personal files of one circuit court judge, Jerome Frank, and two Supreme Court justices, Harold Burton and Felix Frankfurter, the documents released by the FBI under compulsion of the Freedom of Information Act make it possible to reconstruct and analyze the legal dimensions of the Rosenberg case with greater historical accuracy and understanding than before. To investigate the judicial process involved is

⁶ Louis Nizer, *The Implosion Conspiracy* (New York, 1973), 485-95; and Rosemarie Serino, "Espionage Prosecutions in the United States," *Catholic University Law Review*, 4 (1954): 46. Also see Norman S. Beier and Leonard B. Sand, "The Rosenberg Case: History and Hysteria," *American Bar Association Journal*, 40 (1954): 1046-50. Leslie Fiedler, the literary critic and ex-radical, wrote one of the earliest vindications of the government's case, and he also chided American liberals for their naive and sentimental defense of the Rosenbergs in *An End to Innocence* (Boston, 1955), 25-45.

⁷ Even the most exhaustive legal scholarship on the Rosenberg case has usually emphasized one or two issues, particularly the dramatic stay of execution issued by Justice William O. Douglas and the controversy over the applicability of the Atomic Energy Act. Other matters—e.g., the treason clause, the behavior of the chief prosecutor during the trial, and allegations of perjured testimony—have not been discussed. See, for example, two unsigned analyses: "The Rosenberg Case: Some Reflections on Federal Criminal Law," *Columbia Law Review*, 54 (1954): 219-60; and "The Rosenberg Case: A Problem of Statutory Construction," *Northwestern University Law Review*, 48 (1954): 751-59.

⁸ Alfred H. Kelley and Winfred Harbison devote a sentence to the case in their survey, *The American Constitution: Its Origins and Development*, (5th ed., New York, 1976), 827; but Paul L. Murphy, in his *The Constitution in Crisis Times, 1918-1969* (New York, 1972), fails to mention the case, although he discusses other Cold War issues at great length.

⁹ Schneir and Schneir, *Invitation to an Inquest*, esp. 175-212, 237-53.

not, strictly speaking, to invite another inquest. The question of whether Julius and Ethel Rosenberg were “archtraitors” or “martyred saints” cannot be resolved here; what can be judged is the manner in which American legal institutions—especially the Supreme Court—responded to the most politically sensitive litigation of the Cold War era.

THE REFUSAL OF THE SUPREME COURT TO REVIEW THE ROSENBERGS’ CONVICTIONS on at least seven occasions became for Justice Frankfurter one of the gravest moral crises in his lifetime. He wrote several public dissents at the time, but they pale by comparison with the bitterness he expressed privately. “The last days in Washington were not edifying,” he told Herbert Feis in June, 1953, following the Rosenbergs’ execution. “Men’s devotion to law is not profoundly rooted.”¹⁰ The published record before the Supreme Court, he wrote, two weeks before their deaths, “does not tell the story. Indeed, it distorts the story; it largely falsifies the true course of events. What really took place . . . is, on the whole, the most disturbing single experience I have had during my term of service on the Court.”¹¹ Three years later he continued to echo these sentiments to Justice John M. Harlan: “The merits aside, the manner in which the Court disposed of that [the Rosenberg] case, is one of the least edifying episodes in its modern history.”¹²

The legislative origins of the case that so alarmed Justice Frankfurter in the early 1950s go back to World War I. In June 1917, following American intervention into World War I, Congress enacted an omnibus Espionage Act which provided criminal penalties for those who engaged in seditious activities against the war effort or who delivered to any foreign government information relating to the national defense.¹³ Three decades later, during the first summer of the Korean War, the United States indicted four people—the Rosenbergs; Morton Sobell, a friend and college classmate of Julius Rosenberg; and Anatoli A. Yakovlev, a former vice-consul of the Soviet Consulate in New York City—for conspiring to commit espionage from 1944 until 1950. Under the statute, those convicted of such a conspiracy in wartime could be punished by either death or imprisonment for up to thirty years.¹⁴

¹⁰ Frankfurter to Feis, June 29, 1953, Box 35, Frankfurter Papers, Library of Congress (hereafter cited as FFPLC).

¹¹ Frankfurter, “Rosenberg Memorandum,” June 4, 1953, Box 65, File 1, Frankfurter Papers, Harvard Law School Library (hereafter cited as FFP HLS).

¹² Frankfurter to Harlan, October 23, 1956, Box 169, File 15, FFP HLS.

¹³ The pertinent sections of the Espionage Act, 50 U.S.C. 32 (a), 34, as they relate to the Rosenberg case are “(a) Whoever, with intent or reason to believe that it is to be used to the injury of the United States or to the advantage of a foreign nation, communicates, delivers, or transmits or attempts to communicate, deliver, or transmit, to any foreign government . . . or to any representative, officer, agent, employee, subject, or citizen thereof, either directly or indirectly, any document, writing, code book, signal book, sketch, photograph, photographic negative, blueprint, plan, map, model, note, instrument, appliance, or information relating to the national defense, shall be imprisoned not more than twenty years. (b) Whoever violates subsection (a) in time of war shall be punished by death or by imprisonment for not more than thirty years. (d) If two or more persons conspire to violate this section, and one or more of such persons do any act to effect the object of the conspiracy, each of the parties to such conspiracy shall be subject to the punishment provided for the offense which is the object of such conspiracy.”

¹⁴ *Ibid.*

According to the original indictments, the Rosenbergs had been central figures in a Communist spy ring that gave national defense secrets, specifically sketches of high-explosive lens molds and the atomic bomb, to the Soviet Union. Allegedly, David Greenglass, Ethel's brother, who was a machinist at Los Alamos, provided this information between January and September 1945. Eleven of the twelve overt acts charged against the Rosenbergs related to atomic energy secrets, but one dealt with nonatomic, though still classified, documents, which Sobell supposedly obtained from Max Elitcher, an engineer in the navy department, during 1944.¹⁵ Although not included among the overt acts listed in the indictment, David Greenglass also testified that Julius Rosenberg told of stealing the proximity fuse from Emerson Radio, received information "from one of the boys" about a sky platform project in 1947, and gathered other secret material from a contact at General Electric.¹⁶

Elitcher was the only witness to link Sobell to the conspiracy. In building their case against the Rosenbergs, the prosecutors in fact relied upon the testimony of three confessed accomplices—Greenglass; his wife Ruth, who remained unindicted throughout the trial; and Harry Gold, a Philadelphia chemist who testified that he had been a courier for Yakovlev and who had already been convicted for another espionage charge. The physical evidence introduced to buttress this incriminating testimony consisted of only five items, all dubious: drawings of lens molds and an atomic bomb, made from memory by David Greenglass; a photocopy of Gold's hotel registration card from Albuquerque, New Mexico, where, according to Gold and Greenglass, secret documents had been exchanged; a replica of the Jello box lid the Greenglasses said Julius Rosenberg had devised to facilitate the New Mexico meeting; snapshots of the Greenglasses that they claimed were passport photos taken at Rosenberg's urging; and \$4,000 that the Greenglasses said Rosenberg had given them in June 1950 in preparation for their flight from the United States.¹⁷

¹⁵ Supreme Court of the United States, *Transcript of Record: Rosenberg v. United States* (Washington, 1952), c-e.

¹⁶ *Ibid.*, 613, 731-32, 734; and Schneir and Schneir, *Invitation to an Inquest*, 127.

¹⁷ The Greenglasses and Gold, who provided the most damaging testimony against the Rosenbergs, made convincing witnesses because of their extraordinary recollection of the dates, times, and places when information had been exchanged. This precision gave rise, both at the trial and later, to defense charges that they had been carefully coached by the prosecution. In retrospect, it is clear that the Rosenbergs' lawyers, Emanuel and Alexander Bloch, committed major blunders in their cross-examination of the government's star witnesses and in their treatment of evidence. Gold, for example, whose testimony linked Greenglass and therefore Rosenberg to the missing Russian, Yakovlev, was not even cross-examined by the defense. The attorneys accepted at face value Gold's lurid tale of Soviet espionage, of receiving a Jello box lid from Yakovlev and giving the password—"I come from Julius"—to Greenglass, and of his meeting with Greenglass in Albuquerque and the transmission of material back to Yakovlev. Yet, as the Schneirs and others have pointed out, Gold proved to be a less convincing witness in later trials when he endured searching cross-examination. Under questioning by U.S. Attorney Miles Lane, for example, Gold testified that as a Soviet espionage courier "in all cases when I introduced myself I used a false name and in all cases I never indicated my true place of residence." When he met Greenglass in Albuquerque on June 3, 1945 to receive secret information, however, Gold registered at the Hotel Hilton under his real name and occupied a room there for only a few hours before meeting his contact and returning to New York. For a secret agent who during his testimony took great pains to explain all the devious methods of Soviet espionage, Gold's behavior in Albuquerque was indeed strange. In addition, as the Schneirs note, the photocopy of Gold's hotel registration card introduced by the prosecution (and not challenged by the defense) shows two

George Bernhardt, a physician, testified that Rosenberg had sought information in 1950 concerning inoculations required for admission into Mexico. Benjamin Schneider, a photographer, testified that he had taken passport pictures of the Rosenbergs in May 1950 and that Julius had told him "they were going to France." Elizabeth Bentley, an erstwhile Communist party member and highly paid journalist who specialized in exposés of former spies, reported several telephone conversations with a man known to her only as "Julius." The prosecution noted, too, that Julius Rosenberg had been fired from his engineering job with the U.S. Army Signal Corps in 1945 for alleged party affiliation.¹⁸

The Rosenbergs and Sobell denied participation in the conspiracy. Taking the stand in their own defense, the Rosenbergs launched various attacks on their accusers. They denied knowing Gold, Yakovlev, or Bentley. The Greenglasses bore them enmity, they testified, because of family conflicts over their joint business affairs; and David wanted to protect his wife from prosecution and secure a lenient sentence for himself. Julius testified that he attempted to secure information about Mexican inoculation requirements at David's urging and because Ruth had told him that her husband was in trouble for stealing from the army. The Rosenbergs also denied that the photographs taken by Schneider had been passport photos. They both refused on Fifth Amendment grounds to answer questions regarding their membership in the Communist party.¹⁹

contradictory dates, one written in hand by the desk clerk (June 3, 1945) and the other a mechanical time stamp of June 4, 1945 when, Gold testified, he was enroute to New York City. Whether the FBI forged Gold's registration card, as the Schneirs broadly hint, or the double dating was the result of a defect in the Hilton's time-stamp machine, as the FBI later claimed, the Rosenbergs' attorneys obviously missed an opportunity to discredit Gold's testimony and, perhaps, the prosecution's entire case. The Blochs' cross-examination of the Greenglasses was also inept. Attempting to impugn the Greenglasses' character and motives, the Rosenbergs' lawyers failed to probe some rather extraordinary contradictions in the Greenglasses' testimony about their postwar relationships with the Rosenbergs. Their combined business ventures after 1945, for example, failed miserably and produced bad feelings on both sides. Julius attributed the setbacks to David's laziness and incompetence in running the machine shop. Julius finally forced David out of the business. Greenglass placed the blame for their reverses on Rosenberg's arrogance and personal hostility. Yet during this period of family strife, according to Greenglass, his brother-in-law offered to "have the Russians pay for part of my schooling" at the University of Chicago, M.I.T., or New York University "to acquire new friendships with people who were in the field[s] of research . . . like physics and nuclear energy." Although he obviously doubted David's capacity to manage a machine shop, Rosenberg was prepared to bankroll Greenglass's new espionage career at a major university with Russian money, despite the fact that David's entire academic career had thus far consisted of six months at Brooklyn Polytechnic where he had failed eight courses. Finally, the defense did not call its own scientific experts to evaluate Greenglass's sketches. The Blochs accepted the judgment of a single government witness, Walter Koski, an associate professor of physical chemistry, that the drawings were both classified and of great scientific value. *Transcript of Record*, 1150-1230, 656-84, 729-30, 870-73, 254-539, 547-617, 689-1001; and Schneir and Schneir, *Invitation to an Inquest*, 378-90.

¹⁸ The testimony of Bernhardt, Schneider, and the Greenglasses about the Rosenbergs' preparation to leave the United States in 1950 seemed especially incriminating to their case because Sobell had gone to Mexico and faced trial only after unusual extradition proceedings. Sobell claimed that his Mexican trip had been a vacation and that he had been kidnapped by Mexican and American authorities. Although the Rosenbergs admitted that they frequently had "snapshots" taken on family outings, the number of photographs requested from Schneider does seem rather large—"three dozen . . . passport size." On the other hand, Schneider offered confusing testimony about the photographs. He said that he normally charged one dollar for three passport photos but that the Rosenbergs paid him "about nine dollars." And he could produce neither the negatives nor a sales slip to verify the transaction. *Transcript of Record*, 1230-40, 1420-1516, 2124-46; and Schneir and Schneir, *Invitation to an Inquest*, 327-40.

¹⁹ Julius Rosenberg's behavior until the time of his arrest in June 1950 appears somewhat casual for a person whom the government characterized as a master spy, fearful of apprehension, and prepared to flee

On March 29, 1951, following almost a month of testimony, the jury returned a verdict of guilty against all three defendants.²⁰ Seven days later, after consulting two other judges, the chief prosecutor, Irving Saypol, and through Saypol other Justice Department officials, U.S. District Judge Irving Kaufman pronounced sentence: thirty years for Sobell, fifteen years for David Greenglass; death for the Rosenbergs. "I consider your crime worse than murder," Kaufman said, in explanation of the death sentences. "I believe your conduct in putting into the hands of the Russians the A-bomb years before our best scientists predicted Russia would perfect the bomb has already caused the Communist aggression in Korea, with the resultant casualties exceeding fifty thousand and who knows but that millions more of innocent people may pay the price for your treason." "I feel," he continued, "that I must pass such sentence upon the principals in this diabolical conspiracy to destroy a God-fearing nation, which will demonstrate . . . that traffic in military secrets, whether promoted by slavish devotion to a foreign ideology or by a desire for monetary gains must cease."²¹

from the United States. Rosenberg had, according to Greenglass, urged him to leave the country in February, April, twice in May, and again in June, and had provided \$5,000 for the trip. During this same period, however, Julius redoubled his efforts to purchase David's interest in their Pitt Machine Products Co., pressured Greenglass into resigning as a director of the ailing firm on May 1, 1950, and also entered into an agreement with a second partner, David Schein, to repurchase his share in the company. Of course, the government could claim that Rosenberg's intense preoccupation with his business affairs had only been an elaborate charade, designed to give the appearance of normal behavior when he believed himself to be under FBI surveillance and to disguise conveniently travel payments to Greenglass. *Transcript of Record*, 741-42, 745-57, 801-03, 964-67, 1658-60, 1560-1913, 1924-2098.

²⁰ One juror, James A. Gibbons, an accountant for a New York bus company, refused for nearly two days to make the verdict unanimous, not because he believed the Rosenbergs and Sobell to be innocent, but because he did not want to be responsible for the execution of a woman with two small children. The other members of the jury, he later recalled, convinced him "that it wasn't the jury's job to even think about the sentence . . . I felt like Pontius Pilate washing his hands." Most of the jurors, according to Ted Morgan, quickly voted a guilty verdict because they believed the Greenglasses, not the Rosenbergs. "Why would a boy [Greenglass] go to this great length to testify against his sister . . . knowing it might mean their lives?" one asked Morgan. "I could not visualize this happening. I still can't. I felt that he could not have been lying about doing in his own sister." Morgan, "The Rosenberg Jury," 127, 131.

²¹ FBI documents, released to the Rosenbergs' sons and published by the National Committee to Reopen the Rosenberg Case, shed interesting but essentially confusing light upon Judge Kaufman's decision to sentence the Rosenbergs to death, one of the two options permitted under the Espionage Act. On March 16, 1951, before the case went to the jury, a high FBI official, A. H. Belmont, reported informally to the FBI that Kaufman would impose the death penalty upon Julius Rosenberg "if he [Kaufman] doesn't change his mind." On April 3, 1951, however, J. Edgar Hoover received word from his New York supervisor, D. M. Ladd, that Kaufman had consulted Jerome Frank of the court of appeals and District Judge Weinfeld "concerning the sentences he would impose on the defendants." Frank indicated that he was against the death penalty "for any of the defendants . . . Weinfeld indicated that he was in favor of the death penalty for Julius Rosenberg, Morton Sobell, and Ethel Rosenberg." On the day before imposing sentence, according to Saypol's later recollection, Kaufman asked him for his views and urged the chief prosecutor to solicit the opinion of the Department of Justice. "There were differences all around among them," Saypol wrote, "but capital punishment for one or both was in not out." Because of the division in Washington, Kaufman asked Saypol "to refrain from making any recommendation for punishment the next day." These documents refute Kaufman's assertion at the time that he "refrained from asking the Government for a recommendation," but not his other contention that "the Court alone should assume this responsibility." Failing to get a unanimous recommendation from Washington, Kaufman felt at liberty to impose the maximum sentence upon the Rosenbergs, whose crime he characterized as "loathsome." At the end of the trial, Kaufman complimented the jury for "a correct verdict"; and, before passing sentence upon the convicted spies, he recommended that Congress increase the twenty-year penalty for *peacetime* espionage. See Belmont to Ladd, March 16, 1951; Ladd to Hoover, April 3, 1951; Saypol to Clarence M. Kelley, March 13, 1975, all reprinted in "The Kaufman Papers," distributed by the National Committee to Reopen the Rosenberg Case; *Transcript of Record*, 2390-91, 2447-55. Doron Weinberg, past president of the National Lawyers Guild, who wrote an introduction to the FBI documents for the Rosenberg Committee, con-

Thus ended the trial phase of the Rosenberg case, amid the frustration over a bloody military stalemate in Korea, the shock of President Truman's dismissal of General MacArthur, the frenzied construction of fallout shelters, and Senator McCarthy's raucous attacks on Communist subversion in Washington. Over the next two years, in a political atmosphere filled with similar foreign crises and domestic alarms, attorneys for the Rosenbergs brought before the courts a multiplicity of petitions and motions that attempted on both constitutional and statutory grounds to overturn the death sentences and secure a new trial. A few of their legal arguments had little substance—such as the claim that the sections of the Espionage Act under which they had been charged should be declared void as an unconstitutional invasion of speech and press.²² But many of the Rosenbergs' other challenges raised serious issues relating to the fairness of their trial and the appropriateness of their sentences. At one time or another, from the spring of 1951 until June 19, 1953, lawyers for the Rosenbergs posed at least seven substantial objections:

- (1) Their convictions under the Espionage Act should be reversed, it was argued, because they had been secured in violation of Article III, Section 3 of the Constitution which requires that "Treason against the United States shall consist only in levying War against them, or, in adhering to their Enemies, giving them Aid and Comfort. No Person shall be convicted of Treason unless on the Testimony of two Witnesses to the same overt Act, or on Confession in open Court." The Rosenbergs had been charged with a conspiracy to commit espionage, not treason;²³ but throughout the trial they

demanded Kaufman's unilateral discussions with the prosecution as "a flagrant breach of judicial etiquette," a violation of the American Bar Association's Rules of Professional Responsibility, and a subversion of the separation of powers between the judicial and the executive branches of government. *Ibid.*

²² See Justice Reed's opinion for a unanimous Supreme Court in *Gorin v. United States*, 312 U.S. 19 (1941), where the justices specifically rejected a First Amendment attack on the Espionage Act.

²³ It is doubtful that the government could have succeeded with a treason indictment against the Rosenbergs, not only because the courts demanded strict rules of evidence in such cases, but also because the Rosenbergs, according to the government, aided the Soviet Union, with which the United States had never been at war. In *Cramer v. United States*, 325 U.S. 1 (1945), 33–34, the Supreme Court held that "the protection of the two-witness rule extends at least to all acts of the defendant which are used to draw incriminating inferences that aid and comfort have been given Every act, movement, deed, and word of the defendant charged to constitute treason must be supported by the testimony of two witnesses." This high standard of proof led Justice Jackson, the author of the *Cramer* opinion, to suggest that the government would be better advised to employ other legal weapons when attempting to protect national security. "The power of Congress is in no way limited to enact prohibitions of specified acts thought detrimental to our wartime safety," he wrote. "The loyal and the disloyal alike may be forbidden to do acts which place our security in peril, and the trial thereof may be focused upon defendant's specific intent to do those particular acts thus eliminating the accusation of treachery and of general intent to betray which have such passion-rousing potentialities." He specifically cited the Espionage Act as an alternative to an indictment for treason. See 325 U.S. 1, 45. In *United States v. McWilliams*, 54 F. Supp. 791, 793 (Dist. Ct. D.C. 1944), an indictment for treason had been thrown out "since an essential element therein is aid and comfort to 'enemies' and Germany did not become a statutory enemy until December, 1941." Despite abundant rhetoric about a "Cold War," the Soviet Union had not been a "statutory enemy" during the period of the Rosenbergs' activities in 1944–50. *Cramer* and *McWilliams* no doubt suggested to prosecutors that a charge of treason against the Rosenbergs would face many obstacles and that these could be avoided through resort to the espionage statute. The Rosenbergs' lawyers argued, on the contrary, that their clients could be tried only for the offense of treason, relying upon another observation by Justice Jackson in *Cramer*: "Of course we do not intimate that Congress could dispense with the two-witness rule merely by giving the same offense [treason] another name [espionage]." See 325 U.S. 1, 45.

had been branded by the government as “traitors,” and under the Espionage Act they had been convicted for what amounted to treason without the constitutional safeguards required in a treason trial—above all the “two witness” rule. Much of the evidence about their alleged spying, for example, had been provided by a single witness or the uncorroborated testimony of an accomplice.²⁴

(2) Attorneys for the Rosenbergs maintained that to permit imposition of the death penalty for a crime similar to treason (but without the constitutional protections of the treason clause) comprised “cruel and unusual” punishment banned by the Eighth Amendment. The death sentences also violated the Eighth Amendment because no civil court had ever imposed such a penalty under the Espionage Act and the Rosenbergs’ accomplices had received lighter sentences. Finally, Judge Kaufman had abused his discretion in sentencing them to death and the penalty should be reduced.

(3) The prosecution, the Rosenbergs’ attorneys argued, violated chapter 18, section 3432 of the federal criminal code, which requires that a person charged with a capital offense “shall at least three entire days before . . . trial be furnished with . . . a list . . . of the witnesses to be produced . . . for proving the indictment.” Not only did the name of the photographer, Benjamin Schneider, not appear on the list of witnesses for the prosecution, but the government called him to testify *after* the Rosenbergs rested their case.

(4) Judge Kaufman had consistently exhibited hostility toward the Rosenbergs, specifically his refusal to grant a defense motion that the jury reconsider Ruth Greenglass’s testimony on cross-examination in addition to rehearing her testimony for the prosecution. Their lawyers claimed that both portions of the testimony, reheard together, would have permitted the jury to determine whether or not she had been coached by the prosecution.

(5) The Rosenbergs, according to their lawyers, had been denied a fair trial because one witness, Schneider, admitted to giving false testimony and others, including David Greenglass, might have committed perjury within the knowledge of the prosecutors.

(6) The out-of-court behavior of chief prosecutor Irving Saypol deprived the defendants of a fair and impartial trial. With Ruth Greenglass on the witness stand, Saypol’s office announced the arrest of William Perl, a former classmate of Rosenberg and Sobell, on charges of perjury before the grand jury. Perl’s arrest and indictment became front-page news in New York along with the remarks of Irving Saypol. The *New York Times*, for example, reported

²⁴ The Federal Rules of Criminal Procedure permitted a defendant to be convicted upon the uncorroborated testimony of an accomplice, although many states required corroboration of an accomplice’s testimony. “Had the Rosenbergs been tried across the street, in a New York State court where corroboration is required,” one critic noted at the time, “a conviction would have been unlikely on this record.” See “The Rosenberg Case: Some Reflections on Federal Criminal Law,” 233–34.

on March 15, 1951, "Mr. Saypol said . . . that Perl had been listed by the government as a potential witness in the current atomic espionage trial. His intended role on the stand, Mr. Saypol added, was to corroborate certain statements made by David Greenglass and the latter's wife, who are key government witnesses in the trial." Despite Saypol's assurances that Perl's indictment had not been timed to influence the trial, the Rosenbergs claimed that his conduct violated their right to due process of law guaranteed by the Fifth Amendment.

(7) Finally, the indictment, trial, and sentence had been secured under the wrong law. In 1946 Congress provided specific penalties for espionage activities relating to atomic secrets in the Atomic Energy Act. That statute allowed judges to impose the death penalty, but only when the jury so recommended and when the offense had been committed with intention to injure the United States, which are findings not required by the Espionage Act. Since the indictment alleged a conspiracy lasting until 1950, their lawyers contended that the government should have been bound by the requirements of the 1946 law.²⁵

On February 25, 1952 the Court of Appeals for the Second Circuit, in an opinion by Judge Jerome Frank, who had earlier advised Kaufman not to impose the death penalty, affirmed the Rosenbergs' conviction and sentence. Six weeks later, the same court denied their motion for a rehearing.²⁶ In so doing, the three-judge panel, including Tom Swan and Harrie Chase, rejected the first four grounds for a new trial outlined above, but also suggested that its rulings deserved consideration by the Supreme Court of the United States. The tone of the opinion reflected divisions on the circuit court over several issues relating to the Rosenbergs, and the judges split 2-1 in affirming Sobell's conviction, with Frank dissenting.²⁷

²⁵ *Transcript of Record*, 2498-2589; and Schneir and Schneir, *Invitation to an Inquest*, 180-84, 196-212, 238. Section 10(b) (2) of the Atomic Energy Act of 1946, 42 U.S.C. 1810, provides that "Whoever, lawfully or unlawfully, having possession of, access to, control over, or being entrusted with, any document, writing, sketch, photograph, plan, model, instrument, appliance, note or information involving or incorporating restricted data—(A) communicates, transmits, or discloses the same to any individual or person, or attempts or conspires to do any of the foregoing, with intent to injure the United States or with intent to secure an advantage to any foreign nation, upon conviction thereof, shall be punished by death or imprisonment for life (but the penalty of death or imprisonment for life may be imposed only upon recommendation of the jury and only in cases where the offense was committed with intent to injure the United States); or by a fine of not more than \$20,000 or imprisonment for not more than twenty years, or both; (B) communicates, transmits, or discloses the same to any individual or person, or attempts or conspires to do any of the foregoing, with reason to believe such data will be used to injure the United States or to secure an advantage to any foreign nation, shall, upon conviction, be punished by a fine of not more than \$10,000 or imprisonment for not more than ten years, or both."

²⁶ *United States v. Rosenberg*, 195 F. 2d 583 (1952). See note 21 above.

²⁷ Sobell deserved a new trial, Frank argued, because Judge Kaufman had not permitted the jury to determine whether he had joined the Rosenberg-Greenglass-Gold conspiracy relating to atomic secrets or had participated only in a second, distinct conspiracy involving nonatomic secrets. The sole testimony linking Sobell to Rosenberg had been provided by only one witness, Elitcher, who alleged that Sobell and Rosenberg asked him to provide information from the navy department. In Frank's opinion Elitcher's testimony was not sufficient to prove a single, unified conspiracy embracing Sobell, Rosenberg, Greenglass, and Gold. *Ibid.*, 601-02.

Frank's law clerk had mounted a vigorous effort on behalf of the Rosenbergs. In a long, passionate memorandum he called their sentences unjust and accused Kaufman of vindictive behavior.²⁸ He urged Frank to meet the penalty issue directly by reducing the death sentences rather than overturning the convictions on procedural grounds.²⁹ At the same time, he outlined possible grounds for a new trial based on Kaufman's refusal to grant the defense motion concerning Ruth Greenglass's cross-examination testimony. He attributed Kaufman's refusal to extreme prejudice against the defendants.³⁰

Frank did not take his clerk's advice and voted to affirm the Rosenbergs' conviction and sentences. His opinion, however, invited review by the Supreme Court. Indeed, he virtually begged the Court to resolve his own doubts. With Chase and Swan, Frank ruled against the cross-examination point, but he parted with them on three other issues: the use of Schneider as a government witness; the power of appellate courts to modify a death sentence; and the relationship between that penalty, the treason clause, and the Eighth Amendment. Swan and Chase, for example, held that section 3432 of the federal criminal code did not apply to so-called rebuttal witnesses, a category into which they neatly placed Schneider. Frank, on the other hand, argued, "It might well have been error to refuse a reasonable request for adjournment. . . . But defendants made no such request."³¹

The death sentences, according to Swan and Chase, could not be reduced by an appellate court when imposed by a trial judge acting under a valid statute. Frank remained more skeptical. He noted that appellate courts modified financial penalties in civil cases where the zeal of a particular judge led to unjust, inflated awards. He questioned whether defendants should receive less protection in a capital case, and, to support this thesis, he cited an obscure section of the United States Code (2106), dating back to the original Judiciary Act of 1789, that gave both appellate courts and the Supreme Court the power "to affirm, modify . . . , or reverse a Judgment." No decision of the Supreme Court, he concluded, "seems to have cited or considered this statute in passing on the question of the power to reduce a sentence. . . . It is clear that the Supreme Court alone is in a position to hold that . . . 2106 confers authority to reduce a sentence." Frank then added a devastating footnote: "Had this court such power [to reduce the sentence], it might take into consideration the fact that the evidence of the Rosenbergs' activities . . . came almost entirely from accomplices." Those accomplices, including Gold and Greenglass, had all received lighter sentences than the Rosenbergs and one, Ruth Greenglass, had not even been indicted.³²

²⁸ "Memo on Rosenberg, et al.," (n. p., n. d.), 1, Box 105, Rosenberg File, Jerome Frank Papers, Yale University (hereafter cited as JFPY). (Restrictions imposed by Judge Frank and his literary executor prohibit direct quotation from Frank's circuit court papers.)

²⁹ *Ibid.*, 5.

³⁰ *Ibid.*, 3. Also see "The Court's Refusal to Ask the Jury Specifically Whether They Wanted to Hear Read the Cross Examination of Ruth Greenglass, When Requested to do so by the Defense. Constitutes Reversible Error" (n. p., n. d.), 4, Box 105, Rosenberg File, JFPY.

³¹ *United States v. Rosenberg*, 195 F. 2d 583 (1952), 600.

³² *Ibid.*, 605-06, 607 (n. 30).

Finally, Frank urged the Supreme Court to review the case in order to clarify important constitutional issues relating to the treason clause, the espionage statute, and the Eighth Amendment. He rejected the Rosenbergs' arguments that their convictions under the Espionage Act violated Article III, Section 3 of the Constitution. Conspiracy to commit espionage and treason, he noted, were distinct offenses; and in *Ex parte Quirin*, the 1942 Nazi saboteurs case, the Supreme Court had upheld the death penalty for a crime more specific than treason.³³ On the other hand, Frank quoted critics of the *Quirin* decision and pointed out that the Court had not resolved whether or not the death penalty violated the prohibition against "cruel and unusual" punishment when imposed for a crime similar to treason. The Supreme Court, he added wistfully, "may well think it desirable to review that aspect of our decision in this case."³⁴

The Supreme Court, however, on June 7, 1952, rejected Frank's overtures by refusing to hear the Rosenbergs' case, and five months later, the Court also denied the petitions for a rehearing. The brief order denying review noted merely that Justice Hugo Black believed the petitions should have been granted. Justice Frankfurter wrote a separate, public memorandum. He explained that the rejection meant "there were not four members of the Court to whom the grounds . . . seemed sufficiently important." Unwilling to disclose his own vote, he nonetheless took a position on one issue raised in Frank's opinion for the circuit court: "A sentence imposed by a . . . district court," he wrote, "is not within the power of this Court to revise."³⁵

In addition to Black, two other justices had actually voted to hear the case in the secret conferences of June 7 and November 8: Frankfurter and Harold Burton. Black, according to Frankfurter's account of the June 7 conference, "thought the fact that a death sentence had been imposed in time of peace for what was in effect a charge of treason . . . without observance of the constitutional requirement . . . presented a serious question." Burton's notes on the same conference indicate that he wished the Court to consider three aspects of the case: (1) Sobell's conviction; (2) the validity of the death penalty; (3) the treason clause. Frankfurter made "an extended argument" in favor of review-

³³ *Ibid.*, 610-11. *Ex parte Quirin et al.*, 317 U.S. 1 (1942). Justice Jackson had reiterated the distinction between treason and other crimes involving national security in *Cramer v. United States*. See note 23 above. During the drafting of the Constitution, Rufus King had also made the observation that "the legislature might punish capitally under other names than Treason." See Willard Hurst, *The Law of Treason in the United States* (Westport, Conn., 1971), 131.

³⁴ *United States v. Rosenberg*, 195 F. 2d 583 (1952), 611. Clearly, Frank did not find much merit in these arguments. He noted that no court had ever found a sentence imposed under a valid law to be "cruel and unusual." The Rosenbergs' claim, in addition, presumed that Congress would always punish treason with death and that therefore such a penalty for the lesser crime of espionage amounted to "cruel and unusual" punishment. In fact, Congress had made treason punishable by fine and imprisonment as well as death and recent treason cases had not involved the death penalty. See *Cramer v. United States*, 325 U.S. 1 (1945), and *Haupt v. United States*, 330 U.S. 632 (1947). Frank deplored the sentence in the Rosenberg case and no doubt hoped that the Supreme Court would review the case, but on November 18, 1952 he wrote to Zechariah Chafee of the Harvard Law School, "the defendants received a fair trial. Indeed, it was more fair than many in which convictions have been affirmed However, if our court had had power to modify the sentence, I would have voted to do so because . . . testimony was given in circumstances that would not have convinced me sufficiently to serve as a basis for a death sentence." See Frank to Chafee, November 18, 1952, Box 35, File 21, Chafee Papers, Harvard Law School (hereafter cited as CPHLS).

³⁵ *Rosenberg et al. v. United States*, 344 U.S. 838, 850, 889-90 (1952).

ing the case, after both Stanley Reed and the chief justice, Fred Vinson, voted against the petitions. "I expressed no view on the merits," he wrote, but "the rare cases in which federal courts imposed death sentences should generally, I said, be reviewed by us." "We ought," he continued, "to be moved by the fact that . . . [Judge] Frank . . . had given expression to a hope that we would take the case. . . . It was in the public interest to put such doubts to rest, and we alone could do it."³⁶

Three other justices, however, Tom Clark, Sherman Minton, and Robert Jackson, voted against review. Jackson, Frankfurter noted, "saw no point in the case which . . . could possibly lead to reversal. That being so, he [Jackson] thought the principal consideration was not to permit proceedings in the case to drag out." Justice William O. Douglas, therefore, cast a decisive vote. "His 'deny's' are usually curt and unaccompanied by argument," Frankfurter wrote. "His 'deny' this time was unaccompanied by argument. But it was uttered with startling vehemence." The same 6-3 division, one vote short of the four required to grant review, persisted in the conference of November 8 when the justices turned down the Rosenbergs' petition for a rehearing. "Douglas again announced his 'deny,'" Frankfurter remarked, "with unwonted vehemence." Burton's diary notations about the meeting confirm much of Frankfurter's account: "I voted, on 1st pet. [petition] . . . to grant (especially to hear argument on treason issue), but at that time Douglas voted to deny [the petitions]."³⁷

With the exception of Burton and Douglas, the alignment of the Court on June 7 and November 8 is not difficult to understand. Vinson, Clark, and Minton, whom a reporter once described as Harry Truman's law firm, had not usually exhibited intense concern for the rights of defendants who were members of extremist political groups.³⁸ Reed and Jackson, the Court's brilliant eccentric, generally demonstrated more sensitivity to such problems, except in the case of the Communist party where both had compiled less than enviable records.³⁹ Burton, a militant advocate of civil rights for racial minor-

³⁶ Frankfurter, "Rosenberg Memorandum" (FFPHLS), 1-2; and Burton, Conference Sheets, Certs, and Appeals for 1952 Term, Box 248, Harold Burton Papers, Library of Congress (hereafter cited as HBPLC). Frankfurter's brief summary of Black's concerns suggests that the latter was prepared to reconsider his position on certain aspects of the treason clause. Black had joined the Court's opinion in the Nazi saboteurs case, *Ex parte Quirin et al.*, 317 U.S. 1 (1942), and had supported Justice Douglas's dissent against Jackson's restrictive interpretation of the treason clause in *Cramer v. United States*, 325 U.S. 1 (1945), 48-67.

³⁷ Frankfurter, "Rosenberg Memorandum" (FFPHLS) 1-2; and Burton, Rosenberg Memorandum, June 19, 1953 in Diary, June 18 and 19, 1953, Box 248, HBPLC.

³⁸ Clark, for example, had been Truman's first Attorney General from 1945 to 1948. He spearheaded the administration's drive against the "Red menace" in American society, compiled the government's official list of subversive organizations, and launched the successful prosecution of twelve Communist party leaders under the Smith Act. Vinson, of course, had written many of the Court's uncompromising anti-Communist opinions between 1948 and 1952, including *Dennis v. United States*, 341 U.S. 494 (1951), where the justices upheld the convictions of CPUSA leaders for conspiring to advocate the overthrow of the government. See Robert Griffith and Athan Theoharis, *The Specter: Original Essays on the Cold War and the Origins of McCarthyism* (New York, 1974), 84, 179, 188; and Murphy, *The Constitution in Crisis Times*, 252-60, 262-68, 297-309.

³⁹ Murphy, *The Constitution in Crisis Times*, 266-67, 295, 306. Jackson, one of the five remaining New Dealers on the Vinson Court, generally wrote liberal opinions—e.g., *West Virginia State Board of Education v. Barnette*, 319 U.S. 624 (1943), which overturned the compulsory flag salute on First Amendment grounds; and *Cramer v. United States*, 325 U.S. 1 (1945), which imposed strict limitations upon treason prosecutions.

ities did not show similar passion for political ones. His vote to hear the Rosenberg case arose as much from judicial courtesy as from a strong belief in the merits of their petition. "Two justices seemed to have strong feelings," Frankfurter reported Burton as saying on June 7. "He would join them [Black and Frankfurter] for a grant." Candidly, Burton described his own position: "In each instance I was ready to vote against their [the Rosenbergs'] proposals if a vote was necessary, yet I would prefer to give them a chance for argument before the Court acted."⁴⁰

According to Frankfurter's notes, Black seems to have been the only member of the Court with a clear civil libertarian analysis of the case rooted in his concern about the treason clause. Frankfurter's own anxiety at this point arose largely from his belief in the Court's institutional responsibility to hear the case and his desire to calm popular alarm over the legality of the convictions. "I had been reinforced in my views," he wrote of the November 8 decision. "I pointed to heightened public feeling, not the irrational passions aroused in and by the Communists . . . but the disquietude of impartial men of good will."⁴¹ The alliance of Black and Frankfurter on the Rosenberg case was hardly fortuitous, although they had found themselves on the opposite side of most constitutional issues for over a decade. The senior justice from Alabama—regarded as the leader of the Court's liberal, activist wing in the 1940s—preached a vigorous doctrine of legal positivism which required that every action by public officials flow from an unambiguous statutory or constitutional provision. Black abhorred discretionary power, whether manifested in the equity jurisdiction of courts, the rule-making authority of regulatory commissions, or grandiose claims to "inherent authority" put forth by the president. In addition, he had spoken out many times in defense of persecuted ideological minorities.⁴² Frankfurter, the principal spokesman for judicial restraint on the Court, had aroused the concern of civil libertarians largely because of his conservative views on the First Amendment, but he held very fastidious notions about federal criminal procedure and the duty of the

But like Justice Frankfurter, his closest friend on the Court, he manifested a deep preoccupation with social cohesion, which led him at times to sanction rather repressive state controls over unpopular political or social minorities. A former attorney general and chief prosecutor of Nazi war criminals, Jackson also believed that the community had to defend public order by rooting out conspiracies, whether organized by businessmen attempting to subvert the antitrust laws, Jehovah's Witnesses bent upon a disruptive proselytizing campaign, German stormtroopers, or the American Communist party. See his views in *Murdock v. Pennsylvania*, 319 U.S. 105 (1943), and *Martin v. City of Struthers*, 319 U.S. 141 (1943), 171–82; also see his concurring opinion in *Dennis v. United States*, 341 U.S. 494 (1951).

⁴⁰ Frankfurter, "Rosenberg Memorandum" (FFPHLS), 2; and Burton, Rosenberg Memorandum (HBPLC), June 19, 1953.

⁴¹ Frankfurter, "Rosenberg Memorandum" (FFPHLS), 2.

⁴² On Black generally, see Gerald T. Dunne, *Hugo Black and the Judicial Revolution* (New York, 1977), 224–94; and G. Edward White, *The American Judicial Tradition: Profiles of Leading American Judges* (New York, 1976), 221–36, 328–39, 346–65. On his judicial positivism, see especially *Board of Commissioners of the County of Jackson, Kansas v. United States*, 308 U.S. 343 (1939), 354, which denied the courts the authority to make financial restitution to Indians without statutory authorization; and *Youngstown Sheet and Tube Co. v. Sawyer*, 343 U.S. 579 (1952), which denied the president the authority to seize steel mills without congressional sanction. See also his vigorous defense of the First Amendment in the Smith Act-Communist Party case, *Dennis v. United States*, 341 U.S. 494 (1951).

Supreme Court to supervise the administration of justice in the federal courts.⁴³

Justice Douglas, a paladin among American liberals, was the true anomaly. He had consistently joined Black in First Amendment cases and had written several free-speech opinions that other justices regarded as invitations to anarchy. The Court's youngest member at 54, Douglas also maintained an active public life off the bench through lectures, books, and travel geared to his dwindling constituency among the old New Dealers in the Democratic party, some of whom in the summer of 1952 still looked to him as a viable presidential or vice-presidential candidate. A *Nation* magazine poll, published on May 10, 1952, indicated that two-thirds of its readers favored Douglas as the party's candidate.⁴⁴ If Douglas's ambitions for political office remained alive, a confrontation in the Court over domestic espionage amid the fetid atmosphere of 1952 would have presented a dilemma best avoided by refusing to hear the Rosenberg case. Yet Douglas made no effort to secure the Democratic nomination, did not possess a coherent political organization, and still voted against a rehearing in the Rosenberg case three days after Eisenhower's landslide victory in November. A more plausible, if still unflattering, explanation of his behavior is that Douglas, unlike Black and Frankfurter, believed the case then presented neither significant constitutional issues nor a threat to the Court's moral authority.⁴⁵

⁴³ On Frankfurter generally, see C. Herman Pritchett, *The Roosevelt Court: A Study in Judicial Politics and Values, 1937-1947* (New York: 1948), 132-35, 280-84; and White, *The American Judicial Tradition*, 325-56. Certainly Frankfurter could not be accused of harboring sentimental feelings about the Communist party. In addition to concurring in the *Dennis* case, he had voted to uphold the deportation of William Schneiderman and Harry Bridges as well as the anti-Communist provisions of the Taft-Hartley Act. See *Schneiderman v. United States*, 320 U.S. 118 (1943); *Bridges v. Wixon*, 326 U.S. 135 (1945); and *American Communications Association v. Douds*, 339 U.S. 382 (1950). But, in contrast, in decisions involving the administration of justice, see, for example, his opinion in *McNabb v. United States*, 318 U.S. 332 (1943), which reversed two murder convictions because federal officers secured confessions prior to bringing the suspects before a magistrate, and his support of Douglas in *Nye et al. v. United States*, 313 U.S. (1941), which curbed the contempt powers of federal judges, although he refused to sanction a similar limitation upon state courts in *Bridges v. California*, 314 U.S. 252 (1941), 291-300.

⁴⁴ Murphy, *The Constitution in Crisis Times*, 188; and see especially Douglas's opinion in *Terminiello v. Chicago*, 337 U.S. 1 (1949), and the dissents by Frankfurter and Jackson. For the poll and Douglas's political ties, see, for example, *Nation*, May 10, 1952, pp. 444-45, and "Justice Douglas Is Available," *Nation*, January 26, 1952, p. 73. "Think of the rubbishy stuff that so good a person as Freda Kirchway prints . . . that Douglas is available," Frankfurter wrote to Charles C. Burlingham. "What does she really know about him, except what he has told about himself? And has she any realization at all of what it means to have political considerations, consciously or unconsciously, enter into a court that is entrusted with the ultimate disinterested say in the vital affairs of this nation?" Frankfurter to Burlingham, January 25, 1952, Box 36, FFPLC. Frankfurter, of course, tended to be sanctimonious about his colleagues, especially Black and Douglas. The latter he regarded as a ruthless opportunist and judicial chameleon who had used his seat on the Court as a springboard to higher political office and had twisted legal doctrine in order to further these ambitions. Jackson held similar feeling about his youngest colleague; and, in addition, he blamed Douglas for joining with Black to deny him the chief justiceship in 1946. See *From the Diaries of Felix Frankfurter*, ed. Joseph P. Lash (New York, 1974), 161, 173-76, 177-82, 226-27, 283, 301-02, 338, 342-43; and Pritchett, *The Roosevelt Court*, 26-29.

⁴⁵ Douglas and Black had strong civil libertarian records in First Amendment cases that involved speech, press, and association but rather mixed records in national security cases where patriotic emotions tended to run high. In *Viereck v. United States*, 318 U.S. 236 (1943), 253, for example, they dissented when the Court reversed the conviction of a German agent for failure to register with the secretary of state and to disclose his propaganda activities on behalf of the German government. The Court held that Viereck was not

Although Ethel Rosenberg denounced the “pusillanimous rottenness” of the Court’s decision not to hear the case, her husband believed that the action would finally generate an avalanche of protest among progressives and liberals “to grant us our day in court.” Progressives in the legal profession, however, were far less optimistic. “Who will save the Rosenbergs from death?” Burlingham asked Frankfurter after the denial of review. “The British gave [Klaus] Fuchs only 14 years or less. Canada gave its traitors only a few short years. The R’s [Rosenbergs]’ treason was when we were friends of Russia. We must not go back to the 16th Century.”⁴⁶ Following the Supreme Court’s action, Judge Kaufman scheduled the Rosenbergs’ execution for the week of January 12, 1953.

REBUFFED BY THE NATION’S HIGHEST COURT, THE COUPLE’S LAWYERS INSTITUTED new proceedings before U. S. District Court Judge Sylvester Ryan early in December 1952. In their petition they alleged principally that Saypol’s conduct during the Perl indictment and false statements by Schneider entitled their clients to a new trial. On the witness stand Schneider testified that he had not seen the Rosenbergs since 1950 when they came to his photography shop to pick up passport photos. After the trial, however, Schneider admitted that he had been brought into Kaufman’s courtroom on the day before his testimony by FBI agents. The Rosenbergs’ lawyers urged Judge Ryan at the very least to hold a hearing on these issues and receive oral testimony. He rejected their pleas, finding “no relevant or material issue of fact . . . which requires a hearing . . . or which renders the taking of oral testimony either necessary or helpful.”⁴⁷

Schneider had not committed perjury, Ryan argued, merely because he testified that the “last time” he saw Julius Rosenberg had been in the spring of 1950, when in fact Schneider had seen the defendant the day before he testified in court. “There was no motive for falsehood on the part of Schneider and there is not the slightest evidence that Schneider’s testimony on this was intentionally false.” With equal speed Ryan disposed of the Perl indictment: “There is not the slightest proof that any of the trial jurors read of the arrest or indictment . . . or that it came to their attention in any manner.” And, in

required under the law to disclose activities undertaken on his own behalf and reprimanded the government prosecutor for inflaming the jury with his remarks: “It is a fight to the death. The American people are relying upon you ladies and gentlemen for their protection against this sort of a crime. . . . We are at war. You have a duty to perform here.” In his dissent, Douglas characterized the prosecutor’s remarks as merely “stirring eloquence” which “cannot convict him of hitting foul blows.” Likewise, in *Cramer v. United States*, 325 U.S. 1 (1945), 67, Douglas and Black attacked Jackson’s opinion on the treason clause as one that “makes the way easy for the traitor, does violence to the Constitution, and makes justice truly blind.”

⁴⁶ Meeropol and Meeropol, *We Are Your Sons*, 151–52; Burlingham to Frankfurter, November 17, 1952, Box 36, FFPLC. “As a matter of justice,” Zechariah Chafee wrote to Jerome Frank, “a death sentence for spying in war should . . . be enforced only when the secrets were given to an enemy in the war. Here they were given to an allied [sic]. . . . It is absurd to punish the betrayal to Russia in 1944 or 1945 with death when a similar betrayal today, which would be far more injurious to the United States, would be punished only with life imprisonment.” Chafee to Frank, November 14, 1952, Box 35, File 21, CPHLS.

⁴⁷ *United States v. Rosenberg*, 108 F. Supp. 798 (1952), 799–800.

addition, he noted, "The petitioners . . . elected not to move for a mistrial; they may not now object."⁴⁸

Three weeks later, on December 31, 1952, the Circuit Court of Appeals affirmed Ryan's decision, but not without serious reservations expressed privately by Jerome Frank and more openly by one of the senior judges, Tom Swan. In his memoranda to the other members of the court, Frank condemned Saypol's behavior at the time of the Perl indictment. The chief prosecutor, he wrote, told the press what he did not attempt to prove in court: (1) that the government had planned to use Perl's testimony to corroborate the Greenglasses' story; (2) Perl had later backed out; and (3) on this account Perl had been indicted for perjury. If the Rosenbergs' lawyers had then moved for a mistrial, he concluded, Judge Kaufman would have been obliged to grant it. No such motion had been made, however, and this point became decisive for Frank: the defendants could not, after an adverse verdict, obtain a new trial because of misconduct they had elected to ignore. Although their lives were at stake, Frank doubted they could succeed with the argument that their lawyers had made an unwise choice during the trial. Swan's final opinion for the Court followed Frank's analysis. He branded Saypol's conduct as "wholly reprehensible"; but the Rosenbergs had not requested a mistrial, he concluded, and "there is no allegation or evidence that any juror read the newspaper story."⁴⁹

Having lost for a second time in the circuit court, the defense strategists for the Rosenbergs now pursued three related efforts in order to save the couple from execution. The lawyers prepared an appeal of Swan's ruling to the Supreme Court, applied to Judge Kaufman for a reduction in the sentences, and petitioned President Truman for clemency. On January 2, 1953, although Kaufman granted a stay of execution pending a clemency decision from the White House, he refused to modify the sentences. The death penalty would "serve as an example to those who may . . . be tempted to commit similar acts," he argued, and it would protect America against "the home grown and foreign variety of spies, which are and will be a continuing threat to our security." The Rosenbergs, he concluded, had not shown any remorse for their crime and he would stand firm against "a mounting organized campaign of vilification, abuse and pressure" to set aside the sentences.⁵⁰ Harry Truman left office on January 20 without acting on their clemency petition or the thousands of pleas for mercy, including one from Albert Einstein, that poured into the White House. Truman, of course, had endured four years of Republican invective that his administration coddled subversives, lost China to the

⁴⁸ *Ibid.*, 806, 804. Of course, one might argue that the relevance of the Perl indictment could only be determined by a hearing. The Rosenberg jury was not sequestered during the trial, and one member reported to Ted Morgan, "You can shut yourself off to a certain extent, but I'd be riding the subway, and I'm a guy that likes to read sports, you're bound to see a newspaper. . . . Anyone who tells you he can shut himself off completely during a trial has never served on a jury." See Morgan, "The Rosenberg Jury," 126-27.

⁴⁹ Jerome Frank, "United States v. Rosenberg," December 27, 1952, 3-4, Box 105, Rosenberg File, JFPY; and *United States v. Rosenberg*, 200 F. 2d 666 (1952), 670.

⁵⁰ *United States v. Rosenberg et al.*, 109 F. Supp. 108 (1953), 115-16.

Communist hordes, and encouraged Red aggression in Korea. After he had begun prosecutions against Communist party leaders and then the Rosenbergs, it was unlikely that he would commute their sentences and provide the opposition with still another example of the Democrats' "softness" on Communism. Nor could the views of his successor be in doubt. Eisenhower, whatever his private doubts about the lunatic fringe in his own party, had restored the Republicans to power with a campaign that stressed the Democrats' failure to defeat Communism at home and abroad. On February 11, he tersely rejected the Rosenbergs' petition for clemency on the grounds that they had "betrayed the cause of freedom for which free men are fighting and dying at this very hour."⁵¹

The Circuit Court of Appeals for the Second Circuit provided the one ray of hope for the Rosenbergs in an otherwise dark January and February. On February 17, over the objection of government attorneys, the judges stayed the couple's execution until after the Supreme Court had acted on their petition to review Swan's decision. Although he had joined Swan's opinion, Jerome Frank noted that the argument put forth by the defense had merit, "and for my part, I believe the Supreme Court should hear it."⁵² Learned Hand, perhaps the second circuit's most cautious jurist, chastised the government for resisting the stay of execution. "People don't dispose of lives," he said, "just because an attorney didn't make a point . . . There are some Justices on the Supreme Court on whom the conduct of the Prosecuting Attorney might make an impression. . . . Your duty, Mr. Prosecutor, is to seek justice, not to act as a time-keeper."⁵³ At least one member of the judiciary, however, was not plagued by doubts and did act as a time-keeper. Judge Kaufman expressed alarm to FBI officials in New York over the circuit court's action. He feared that the Supreme Court might not dispose of the appeal before their traditional June recess, and he urged the Justice Department to "push the matter vigorously" in order to bring it before the high Court.⁵⁴

During its regular conference on April 11, however, the Supreme Court again denied the Rosenbergs' petition for review, thereby allowing Swan's opinion for the circuit court to stand. Judge Hand had been correct: the prosecutor's conduct and Swan's criticism made an impression upon some of the justices, but not enough to force review of the case. Frankfurter's docket book and memorandum of the meeting indicate that only he and Black now voted to hear the petition. Justice Burton, who had urged review six months before, "thought the issues raised in this new proceeding were without merit," while Justice Douglas, according to Frankfurter's notes, also turned down the case "in the same harsh tone."⁵⁵

⁵¹ Schneir and Schneir, *Invitation to an Inquest*, 192; also see 186-93; and Richard M. Fried, *Men Against McCarthy* (New York, 1976), especially 219-53.

⁵² *New York Times*, February 17, 1953, pp. 1, 19, and February 18, 1953, pp. 1, 12.

⁵³ Quoted in Meeropol and Meeropol, *We Are Your Sons*, 187.

⁵⁴ Even FBI officials were surprised by Kaufman's intervention. A. H. Belmont told J. Edgar Hoover that "this is a matter which should be handled by the Department [of Justice] and we should not express an opinion." See Belmont to Hoover, February 19, 1953, "Kaufman Papers."

⁵⁵ Frankfurter, "Rosenberg Memorandum" (FFPHLS), 4; Jules [sic] Rosenberg and Ethel Rosenberg v. United States, No. 687, Docket File 15, Box 67, FFPPLS. Burton's conference sheet for May 2, 1953

The Saypol incident, coupled with Swan's rebuke, stirred in Frankfurter a powerful conviction that the Supreme Court should hear the case and an equally intense frustration that he could do very little about it. Over the objections of Vinson, Clark, Minton, and Reed, the official order denying the petition was held up for more than a month until May 20 as Frankfurter debated whether or not to write a dissent from the Court's refusal to review the case.⁵⁶ In those weeks he weighed several considerations: personal disgust at Saypol's actions, a concern for the Court's future reputation, and fear that he might feed "flames of disquietude and passion and disunity." On the one hand, the death sentences should not be carried out, he told the other justices, "without putting behind" those sentences "the moral authority that would come from a finding by this Court, after an examination of the record and hearing argument, that there was no flaw in the trial that calls for reversal." Unless the Supreme Court acted, he said, doubts about the case would multiply because judges on the court of appeals had made observations "which naturally enough arouse disquietude in minds that are as fiercely hostile to the Communist danger as are Messrs. Jenner, McCarthy and Velde, but who are also concerned for those American traditions which make them hostile to Communism." He could not justify a dissent without describing in detail what he called "Saypol's inexcusable conduct," yet to do so, he reasoned, "might help to make a hero of him [Saypol], as Judge Medina has been made a hero of for conduct in which no English judge would dare to indulge, no matter what his passion or his egotism."⁵⁷

On the other hand, Frankfurter believed that whatever he wrote in dissent might become grist for the propaganda mills of the Communist party or lead "high-minded and patriotic laymen who do not understand these things to believe that I implied that the Rosenbergs were convicted though innocent." Although Frankfurter professed not to be "awed by fear of the puny force of Communist influence in this country," he finally decided not to dissent because he believed radicals would distort his views. Following his discussions with Justice Black, both agreed to append to the denial order a simple paragraph: "Mr. Justice Black and Mr. Justice Frankfurter, referring to the positions they took . . . last November, adhere to them."⁵⁸

After Frankfurter's own ordeal of indecision, the memorandum of May 22 circulated by Justice Douglas announcing his change of position startled Frankfurter and the other justices. That memorandum threw the Rosenberg case and the Court deeper into controversy and confusion than ever before. "I have done further work on this case and given the problem more study," Douglas wrote. "I . . . have reluctantly concluded that certiorari should be granted," he continued. "I ask that the order of denial carry the following notation: Mr. Justice Douglas, agreeing with the Court of Appeals that some

indicates that the petition was turned down on a 7-2 vote, with Burton voting with the majority: See Box 248, HBP.LC.

⁵⁶ Frankfurter to Vinson, May 16, 1953, Box 65, File 7, FFP.HLS.

⁵⁷ Frankfurter, "Memorandum for the Conference," May 20, 1953, Box 65, File 7, FFP.HLS. Harold Medina had served as the trial judge in the *Dennis* case.

⁵⁸ *Ibid.*

of the conduct of the United States Attorney was 'wholly reprehensible' but, believing in disagreement with the Court of Appeals that it probably prejudiced the defendants seriously, votes to grant certiorari."⁵⁹ Douglas, who had on three occasions voted against hearing the case, now proposed to dissent on the merits. Not only had an important question been raised, he argued, but the Rosenbergs had been "seriously" prejudiced and the Supreme Court seemed not to care. At the urging of Frankfurter and others, Vinson agreed to reopen discussion of the case at the regular conference scheduled for May 23.⁶⁰

Stunned by Douglas's "last-minute change of position" but also sensing an opportunity for the Rosenbergs, Frankfurter wrote a moving letter to Burton and caucused briefly with Jackson in the hope of gaining their support to review the case. He quoted to Burton a short poem by Eugene Wambaugh, who half a century earlier had taught constitutional law at the Harvard Law School:

Let not the judgment that is just
Be judged too soon,
But be reserved, if judge one must,
Till noon.
Or yet till Evening, that the way
Repentant may lie open all the day.

Douglas's memorandum, Frankfurter wrote Burton, "put the whole Court in a hole" but that should not prevent them from "examining the hole in order to see whether it is for the good of the Court to remain in the hole." He reminded Burton of both the *Mooney* case—in which Tom Mooney, a California labor leader, had been sentenced to death because of perjured testimony—and the *Sacco-Vanzetti* case, both of which, he said, had stained the highest courts of California and Massachusetts when the judges refused to face questions of potential injustice. Here, too, was such a case, and "we cannot ostrich-like bury our heads in the sand" in a position of being "heedless to the pronouncement of a member of the Court [Douglas] . . . who has created for himself the reputation of being especially sensitive to the claims of injustice."⁶¹

To Jackson, Frankfurter indicated that, if the Court refused to hear the case, he would probably write a dissent of his own so that Douglas's views would not stand alone. Frankfurter did not wish to dissent, he also told Jackson, but Douglas's memorandum made it almost inevitable. Jackson, in turn, commenced a savage attack upon Douglas. "Don't worry," Jackson told Frank-

⁵⁹ Douglas, "Memorandum to the Conference, Re: Rosenberg v. United States; Sobell v. United States," May 22, 1953, Box 248, HBPLC. Certiorari is the legal term for the writ issued by the Supreme Court when it grants review.

⁶⁰ "After having reached what I had assumed was the end of a long and laborious intellectual journey," Frankfurter wrote to the other members of the Court, "I must now, in view of Brother Douglas' memorandum, retrace it in light of the new situation created by that memorandum. . . . Brother Douglas' change of position obviously requires a reopening of the discussion at Conference." Frankfurter, "Memorandum for the Conference," May 22, 1953, Box 248, HBPLC.

⁶¹ Frankfurter to Burton, May 23, 1953, Box 65, File 2, FFPPLS. Also see Richard H. Frost, *The Mooney Case* (Stanford, Calif., 1968).

furter, "Douglas's memorandum isn't going down." It was, Jackson added, "the dirtiest, most shameful, most cynical performance that I think I have ever heard of in matters pertaining to law." If Jackson's meaning was lost upon Frankfurter at the moment, it emerged more clearly in the afternoon conference when the Rosenberg case came up for discussion. Even in the face of Douglas's provocative document which accused them of indifference to due process, five members of the Court—Vinson, Clark, Reed, Minton, and Burton—remained opposed to review. Black, ill at home, had left a vote in favor of taking the case. Douglas now said he would review it and so did Jackson, who administered a sharp rebuke to Douglas and let it be known that he cast his "yes" vote only to quash publication of Douglas's dissent and to extricate the Court from a dilemma. By this time, Jackson noted, four justices had voted for review on different occasions—Black, Frankfurter, Burton, and Douglas. These votes would be leaked to the press and create still more furor over the case. In such circumstances, Jackson saw no alternative but to review it.⁶²

Clearly, Jackson had become a reluctant ally. Several weeks earlier he refused to hear the case, but he told Frankfurter he might join a dissent that reprimanded Saypol. "I cannot imagine," Jackson said, "that you can be too severe on him [Saypol] to suit me." Like Douglas, he had consistently voted against the Rosenbergs' petitions; but now, although he believed his original position to be fundamentally sound, he provided the fourth vote to review Swan's opinion. "Since there were thus four votes," Frankfurter wrote of the meeting, "we proceeded to discuss when the case should be heard." Burton volunteered to cancel his European vacation in order to hold oral arguments on or before July 6, but at this point, according to Frankfurter, Douglas interrupted the discussion: "What he [Douglas] had written was badly drawn, he guessed. He hadn't realized it would embarrass anyone. He would just withdraw his memorandum if that would help matters." Immediately, Jackson announced that he would not vote to grant the petition because Douglas had taken back the offending document and the Court was now where it had been before. The conference dissolved in confusion, with no one, save perhaps Douglas, certain whether he had rejoined those voting to deny review or whether he remained in dissent, even without the memorandum. One thing was certain: for the fourth time, the Rosenbergs had failed to muster the necessary votes. In the hallway outside the conference room, Jackson stopped Frankfurter and said triumphantly with reference to Justice Douglas, "That S.O.B.'s bluff was called."⁶³

On May 25, 1953, the Monday following its stormy conference, the Court released a formal order denying the Rosenbergs' petition. It noted merely that Justice Douglas "is of the opinion [it] . . . should be granted."⁶⁴ Absent was his reference to Saypol's "wholly reprehensible" conduct; missing, too, was

⁶² Frankfurter, "Rosenberg Memorandum" (FFPHLS), 6, 7.

⁶³ *Ibid.*, 8, 9. Burton had scheduled a European vacation for early July. He left on July 11, 1953; see his Diary, July 11, 1953, Box 2, HBPLC.

⁶⁴ *Rosenberg et al. v. United States*, 345 U.S. 965 (1953), 966.

Douglas's conclusion in the memorandum of May 22 that this conduct "probably prejudiced the defendants seriously." Douglas, indeed, had backed down, but the intriguing question remains: why had he done so? Douglas's own explanation, given to and reported by Black to Frankfurter, was that he withdrew the memorandum when it became clear the Court would only grant a hearing on the question of whether or not to grant certiorari. "Of course," Jackson responded, "that is wholly false It wasn't that at all. We voted to grant until Douglas withdrew his memorandum."⁶⁵ The harshest interpretation, provided by Jackson through Frankfurter, suggests that Douglas did not want the case brought before the Court where he, like the others, would be forced to affirm or reverse the convictions. At the same time, many, particularly those of the American Left, believed that the Rosenbergs had not received a fair trial. The memorandum of May 22 provided a way out of his predicament: Douglas could dissent vigorously from the denial of certiorari, affirm his liberal credentials, yet not be required to vote on the case after full arguments. He withdrew the memorandum in the conference when it became clear that the Court, above all Jackson, preferred to hear the case rather than endure a provocative dissent. Sensing Jackson's motives, Douglas retreated, encouraged Jackson to switch his vote, and thereby killed the grant of certiorari.

One must, of course, view with skepticism this interpretation, contained in documents prepared by a justice who from the beginning of their association on the bench found himself at odds, personally and ideologically, with Douglas. Although Frankfurter may have embellished his account, he certainly did not invent Douglas's memorandum of May 22. His careful reconstruction of the conference, moreover, including what lay behind Jackson's vote and the details of Burton's projected trip, also lends credibility to his analysis of Douglas's behavior. Is there another, reasonable interpretation? Perhaps. Douglas, who tended to write his legal opinions quickly, may have composed the dissent of May 22 in haste, encountered withering criticism in the meeting, and retreated without devious motives. If so, regardless of the presence or absence of a formal vote to hear the case (assuming he cared about the Rosenbergs' petition), Douglas becomes a timid poker player whose bluff had been called. His threatened dissent had forced the Court to reconsider its denial of review. He thus entered the conference with a strong hand. And, from all we know of his behavior in other cases, Douglas thrived on tough intellectual combat, enjoyed the role of dissenter, and was not easily intimidated by his colleagues' wrath. Certainly Douglas was no stranger to the rough-and-tumble of Court politics, and he knew how to round up votes. Franklin Roosevelt believed him to be one of the best poker players in Washington. If Jackson's conduct did not exhibit the highest level of judicial integrity, Douglas's remains inexplicable in view of his own later apparent interest in the case.⁶⁶

⁶⁵ Frankfurter, "Rosenberg Memorandum" (FFPHLS), 6.

⁶⁶ In his recently published autobiography, Douglas does not discuss at length his judicial career, with one exception—the Rosenberg case. When recounting the public abuse he suffered while on the Court,

INSTEAD OF SEALING THE ROSENBERGS' FATE, THE SUPREME COURT'S NEGATIVE decision on May 25 served only as a prelude to the tangled legal and political conflicts of the following month as America's atom-spy drama moved to its grim conclusion against a backdrop of Cold War propaganda, diplomatic crises, and military disasters. Like the battleground in Korea, where Red Chinese troops upset the truce lines on June 14 with the heaviest assault in two years, the Rosenberg case had become a minefield for every judge, lawyer, and politician trapped not only in its reality but in its symbolism as well. Justice Frankfurter's memoranda indicate how a normally bold jurist, outraged by the prosecution's tactics, could be intimidated by the prospect that words of dissent could be used against the nation in its global battle against Communism. Justice Black, usually a pugnacious dissenter in cases that touched sensitive political issues, apparently shared some of Frankfurter's fears. It is not surprising that in the final weeks of the Rosenberg case passion often triumphed over reason. Whatever opportunities for reconsideration or compromise may have once existed at either the judicial or executive level quickly vanished in June once the couple refused to confess their guilt and cooperate with authorities in exchange for clemency. The Rosenbergs were thus finally left at the mercy of others: tired and desperate defense lawyers, many of them strangers to the case; a combative trial judge unwilling to back down under any circumstances; an executive branch anxious to rid itself of the whole affair as quickly as possible, but afraid of jarring its allies abroad or tarnishing its anti-Communist image at home; and, finally, a Supreme Court openly split by ideological divisions and torn asunder by personal feuds and hatreds.

Following the Supreme Court's action, Kaufman once again refused to reduce the sentences and rescheduled the Rosenbergs' execution for the week of June 18—a decision upheld by the circuit court without opinion on June 5. Kaufman's "patriotic" rhetoric during and after the trial made him a principal villain in the case, especially for those who believed that the death sentences were inspired more by anti-Communist hysteria and a desire for vengeance than by the need to protect national security in the future. As criticism mounted over his conduct, Kaufman developed a siege mentality and endowed his own role with the romantic aura of a lone judicial crusader against subversion, who, despite Russian propaganda "poisoning the mind of the peoples of the world" and threats against his life, intended to punish America's chief traitors.⁶⁷

Reality in the White House was far more complex. What to do about the Rosenbergs never became the principal question before Eisenhower and his

Douglas twice refers to his votes for a stay of execution in the case but not to his position on earlier petitions for review. See William O. Douglas, *Go East, Young Man* (New York, 1974), 23, 469–70. In a letter to Mr. Justice Douglas on November 26, 1974, I asked him to comment upon the memorandum of May 22. He declined to do so. Instead, he provided copies of the Court's official reports for the case and concluded, "I am puzzled by your inquiry as the journal entry makes everything clear." In my opinion, the official reports do nothing of the kind. William O. Douglas to author, December 19, 1974.

⁶⁷ New York *Times*, June 6, 1953, p. 7; *United States v. Rosenberg et al.*, 109 F. Supp. 108 (1953), 115; and Belmont to Hoover, February 19, 1953, "Kaufman Papers."

staff during early June, except as the couple's case related to other pre-occupations of the Cold War. In addition to the murderous Chinese offensive, the president faced a rebellion by the volatile Syngman Rhee; the South Korean leader released North Korean prisoners in defiance of agreements with the United States, denounced Eisenhower's truce plan as "the death of Korea," and vowed to fight on alone against the Communist menace. On June 17, moreover, Russian tanks put down anti-Soviet demonstrations in East Berlin. Under these circumstances, it is amazing that the administration offered to commute the Rosenbergs' death sentences in return for their confessions and assistance in tracking down other spies—a proposition which the couple spurned on June 2 and the Justice Department denied having made.⁶⁸ But Eisenhower, despite his desire to maintain a resolute anti-Communist image, also recognized that the case had engendered huge protests in England, France, and Belgium and had inspired pleas for commutation from twenty Israeli religious leaders and the Pope. At the same time that it made overtures to the Rosenbergs, the administration flexed its atomic muscles by announcing that the United States had detonated a plutonium bomb twice as powerful as the ones dropped on Japan. The two events were hardly isolated or unrelated to the deteriorating situation in Korea. The "Eisenhower waltz," as Senator Lester Hunt once observed, usually involved "one step forward, two steps backward and one sidestep!"⁶⁹

While Judge Kaufman remained steadfast in his refusal to modify the death sentences and the Eisenhower administration pursued its characteristic strategy of indecision and obfuscation, the Rosenbergs' principal lawyer and friend, Emanuel Bloch, clutched at every legal device that might possibly save the couple. In these efforts Bloch was aided by several enterprising newspaper reporters and a new team of lawyers, including Malcolm Sharp, a professor of law at the University of Chicago, and the legendary John Finerty, a long-time defender of civil liberties, who had unsuccessfully appealed the convictions of Sacco and Vanzetti to the Supreme Court and secured a measure of legal vindication for Tom Mooney. Finerty joined Bloch in presenting the Rosenbergs' second petition to the Supreme Court and there can be little doubt that his presence strongly influenced Justice Frankfurter, who, like Finerty, had played an active role in both the Sacco-Vanzetti and Mooney cases.⁷⁰ On June 6, these lawyers filed a brief with Judge Kaufman detailing the discovery of "new evidence" in the Rosenberg case which, they argued, indicated that the prosecution had knowingly used the perjured testimony of the Greenglasses.

⁶⁸ According to Julius Rosenberg's version of the offer, James V. Bennett, Federal Director of Prisons, suggested on behalf of Attorney General Herbert Brownell that "Judge Kaufman made a terrible blunder with this outrageous sentence and he has the bull of [sic] the tail and he can't let go. . . . He needs you to help him change this sentence and you can do this by telling all you know." Meeropol and Meeropol, *We Are Your Sons*, 211. On events in Korea and East Berlin, see the *New York Times*, June 12, 1953, pp. 1–2; June 15, 1953, pp. 1, 3; and June 18, 1953, pp. 1, 3, 8.

⁶⁹ Hunt as quoted in Fried, *Men Against McCarthy*, 262. Also see Schneir and Schneir, *Invitation to an Inquest*, 180, 192–94; and *New York Times*, June 5, 1953, pp. 1, 11.

⁷⁰ Frost, *Mooney Case*, 289, 303, 401; and Felix Frankfurter, *Felix Frankfurter Reminiscences*, ed. Harland B. Phillips (New York, 1960), 130–39, 202–17.

They asked Kaufman to order a new trial or, at the very least, to hold a hearing during which the “new evidence” could be carefully examined by both the defense and the prosecution.

The “new evidence”—according to Bloch, Sharp, and Finerty—proved that the Greenglasses lied during the trial about three matters: (1) Russian “gifts” to the Rosenbergs; (2) pretrial statements made to the FBI; and (3) the theft of uranium from Los Alamos by David Greenglass. During his testimony, David alleged that the Rosenbergs had received several presents from the Russians as payment for their espionage activities, including watches and a console table. Moreover, David said, “Julius told me that he did pictures on that table.” Ruth Greenglass did not describe the table as a gift from the Russians; but both Rosenbergs, she said, told her it was given to them by a friend. “There was a portion of the table that was hollowed out for a lamp to fit underneath it,” Ruth added, “so that the table could be used for photographic purposes.” Evelyn Cox, the Rosenbergs’ housekeeper during the war years, also testified that Ethel Rosenberg had told her that “a friend of her husband gave it [the console table] to him as a gift.” The Rosenbergs, she added, sometimes kept the table in a closet.⁷¹ In the trial, this testimony regarding the console table had been extremely damaging to the case for the defense. It conveyed to the jury a frightening stereotype of secret agents, crouched over a table in a darkened room, snapping photographs of classified documents. The Rosenbergs denied that the table had been a gift or that it served any special purpose other than as a place where meals were occasionally served. Julius claimed that he purchased the table from Macy’s department store. Macy’s employees could neither confirm nor refute his testimony because their sales records from 1944–45 had been destroyed and the defense did not produce the table in court because they assumed that all of the Rosenbergs’ furniture had been sold following their arrest.⁷²

In the spring of 1953, however, a reporter for the *National Guardian* located the mysterious console table in the apartment of Julius’s mother, Sophie Rosenberg, who had taken custody of the two Rosenberg children and some of the couple’s old household possessions. An illiterate, Sophie Rosenberg had not followed the trial of her son in the newspapers, and she did not, therefore, understand the significance of the console table until the reporter questioned her about it. The defense lawyers introduced before Kaufman photographs of the table, affidavits from the family, and a statement by a Macy’s employee, all of which tended to confirm much of the Rosenbergs’ account and to cast doubt upon the testimony offered by the Greenglasses. The photographs did not indicate that the table had been “hollowed out” underneath and the Macy’s employee, who had been a buyer of furniture for the store in 1944 and 1945, believed it to be one of their “lower-priced tables,” probably sold “sometime during or subsequent to the year 1944.” By themselves, of course, neither the photographs nor the affidavits could prove that the Greenglasses

⁷¹ *Transcript of Record*, 737–41, 900–01, 1014, 2104.

⁷² Schneir and Schneir, *Invitation to an Inquest*, 200–01.

had lied or, more crucially, that the prosecution had known of their perjury. Although the table may not have been “hollowed out” underneath as Ruth Greenglass claimed, her husband had only testified that Julius Rosenberg “did pictures on that table.” The fact that Macy’s sold the table in 1944 or 1945 did not resolve the issue of who had purchased it—the Rosenbergs, a friend, or the Russians? These were issues that could be settled only in the course of a formal hearing where both the table and the witnesses would be subjected to further examination. Kaufman, however, refused to take that step, because, he argued “there would still be lacking any showing that the Government knowingly used perjurious testimony.” He also branded the console table testimony as insignificant and rebuked the lawyers for failing to produce the evidence during the trial.⁷³

The Rosenbergs’ lawyers also sought to persuade Kaufman that many of the Greenglasses’ pretrial statements, made to their attorneys following interrogation by the FBI, contradicted later testimony and indicated that David and Ruth had committed perjury. The attorneys also presented to Kaufman several memoranda prepared by the Greenglasses’ lawyers in July 1950, later stolen from the law firm’s files, and published by *Combat*, a Parisian newspaper, in April 1953. These documents show that David Greenglass told his lawyers he had made “a number of confusing statements purposely in order to confound the FBI and to draw attention from his wife.” What David recounted to the FBI during his initial interviews and then recited to his lawyers was indeed very confusing with respect to his own espionage activities and the roles played by his wife, Harry Gold, and the Rosenbergs. David told his lawyers that “Julius Rosenberg is apparently very close to this whole situation” and added that Rosenberg had once introduced him “to a man in a car somewhere in New York who apparently made this request” to supply information from Los Alamos. On the other hand, Greenglass said to his lawyers, “I told them [the FBI] that . . . my wife asked me if I would give information. I made sure to tell the F.B.I. that she was transmitting this info from my Brother in Law Julius and was not her own idea.” He informed the FBI of his meeting with Gold in Albuquerque, Greenglass said, including the fact that “I identified Gold by a torn or cut out piece of card”; but, he continued, “I definitely placed my wife out of the room at the time of Gold’s visit. Also I didn’t know who sent Gold to me.” In a discussion with lawyers on the day of her husband’s arrest, Ruth Greenglass told them that “she had remembered no visitors at her house in New Mexico, that David had a “tendency to hysteria,” and that “he would say things were so even if they were not.”⁷⁴

The purloined memoranda did not present a *prima facie* case of perjury but did raise serious questions about the Greenglasses’ credibility and their relationship to the FBI and government prosecutors. In his initial interview with the FBI, according to the lawyers’ account, David did not mention his

⁷³ *Ibid.*, 199–200, 202.

⁷⁴ *Ibid.*, 204–07.

sister, Ethel, and attempted to “draw attention from his wife.” He even placed her out of the room during Gold’s visit to Albuquerque, yet on the witness stand Ruth Greenglass told a very different story: “David gave him [Gold] the written sheets of information and we all spoke for a few minutes and we went out for a walk.” David also told his lawyer on June 17, 1950 that he could not remember certain details of Gold’s visit, “but I allowed it in the statement.” When did the Greenglasses begin to fill in the details about Ethel Rosenberg’s espionage activities, about visitors to their home in Albuquerque, and about who sent Harry Gold? Did their memories become more acute under relentless FBI interrogation? Had David attempted to save both his wife and sister, but failed? Had the Greenglasses invented large portions of their testimony in order to please the government and receive lighter punishment? Kaufman brushed aside these considerations. The evidence did not entitle the Rosenbergs to a new trial or a preliminary hearing, he said, because David Greenglass had made six or seven statements to the FBI and may have added information each time. Pretrial documents, he added, did not prove that the Greenglasses testified falsely at the trial.⁷⁵

Finally, the defense presented Kaufman with an affidavit from David’s brother, Bernard Greenglass, who swore, “my brother David told me he had taken a sample of uranium from Los Alamos without permission of the authorities.” Bernard also stated that, prior to David’s arrest in June 1950, his brother indicated that “he had thrown this uranium into the East River.” Bernard Greenglass’s affidavit, the Rosenbergs’ lawyers argued, suggested that David had been involved in a separate espionage offense, feared detection by the FBI, and had sought assistance from Julius Rosenberg, including money and information about smallpox vaccinations. Even if true, Kaufman responded, the theft of uranium did not “provide a motive for perjury, designed to implicate innocent members of Greenglass’s family in this most serious crime.” He studied the defense’s “new evidence” for two days, heard three hours of oral argument, and after a recess of fifteen minutes denied the Rosenbergs a new trial or a hearing. He called their contentions “unsupported and incredible.” Two days later, in a *per curiam* opinion, the court of appeals affirmed this ruling and denied a further stay of execution.⁷⁶

On June 12, with only six days remaining before the scheduled executions and three days before the Supreme Court adjourned for the summer, Bloch and Finerty applied to Justice Jackson for a stay of execution in order to give the defense additional time to prepare the briefs for an appeal of Kaufman’s ruling on the “new evidence.” Jackson hesitated to act alone, but referred their application to the entire Court with a recommendation that the justices hear oral argument on Monday, June 15, the final day of the Court’s regular term. Jackson’s recommendation provoked a sharp division among the justices during their regular conference on Saturday, June 13. He gained the

⁷⁵ *Transcript of Record*, 1002–07; and Schneir and Schneir, *Invitation to an Inquest*, 210.

⁷⁶ *New York Times*, June 9, 1953, p. 13; Schneir and Schneir, *Invitation to an Inquest*, 211–12; and *United States v. Julius Rosenberg and Ethel Rosenberg*, 204 F. 2d 688 (1953).

support of three others to hear oral argument—Black, Frankfurter, and Burton—but Douglas, who had proposed to write a dissent on the basis of injustice several weeks before, again joined the four stalwarts who had consistently voted against the Rosenbergs' efforts to secure review by the Supreme Court. Douglas said that he would grant a stay of execution and hear the case "on the merits" but that "there would be no end served by hearing oral argument on the motion for a stay." Having rejected Jackson's recommendation, the justices also turned down the application for a stay of execution *without* an oral hearing by a 5–4 vote with Justice Burton switching to the majority. Burton, in other words, refused to grant a stay but would hear oral argument on the question. Douglas would grant a stay but dismissed the necessity for any argument. Three justices—Black, Frankfurter, and Jackson—voted for either formula. Douglas's intransigence clearly doomed the application because, although both he and Burton insisted upon all or nothing, Burton asked for much less: a hearing on the application. Justice Black, for example, who never waived in his support of the Rosenbergs' various petitions, agreed with Douglas that the case should be faced on the merits, but he nonetheless supported Jackson's recommendation to hear argument on the question of a stay. A stubborn devotion to principles, Black reasoned, should not foreclose any opportunity to bring the case before the Court.⁷⁷

The justices' weekend deliberations took place against a background of mounting protests on behalf of the Rosenbergs and the unexpected intervention into the case by two lawyers, Fyke Farmer and Daniel G. Marshall, who represented a loose coalition of civil libertarians and church activists, including Irwin Edelman, the author of a critical pamphlet on the case. On Saturday, as pro-Rosenberg pickets demonstrated in front of the White House and thousands of letters reached the president pleading for their lives, Farmer petitioned Judge Kaufman for a stay of execution and a writ of habeas corpus based in part upon an argument that they had been indicted, tried, and sentenced under the wrong law. Kaufman rejected Farmer's efforts on Monday at about the same time that the Supreme Court announced its refusal to stay the executions or hold oral argument on the application. Kaufman dismissed Farmer as an "intruder" and "interloper," who was attempting to insinuate himself into the litigation without the consent of the Rosenbergs or their attorneys.⁷⁸

On Monday the Court prepared to adjourn after turning down Jackson's recommendation, but Finerty quickly moved to file an original writ of habeas corpus, based upon the Saypol incident and the perjury issues raised before

⁷⁷ *Rosenberg et al. v. United States*, 345 U.S. 989 (1953). Also see Frankfurter to the Conference, June 15, 1953, Box 65, File 2, FFPHLS; and Burton to the Conference, June 15, 1953, Box 65, File 3, FFPHLS.

⁷⁸ *New York Times*, June 14, 1953, pp. 1, 30; and June 16, 1953, pp. 1, 19. Over 6,000 pickets paraded before the White House that weekend. On June 12, Harold Urey, the distinguished nuclear scientist, condemned the government's case as an outrage against logic and justice. "A man of Greenglass's capacity," he said, "is wholly incapable of transmitting the physics, chemistry, and mathematics of the atomic bomb to anyone." The "new evidence" presented to the courts, he told President Eisenhower, made it plain that the prosecution depended upon "the blowing up of patently perjured testimony." *New York Times*, June 13, 1953, pp. 1, 8.

Kaufman and the court of appeals. He carefully structured his arguments around successful pleadings in the *Mooney* case, in which the Supreme Court had entertained, on a writ of habeas corpus, the claim that knowing use of perjured testimony by the prosecution deprived the accused of due process and justified a new trial. In conference that afternoon, the justices voted to deny Finerty's application, although Black dissented and Frankfurter argued for what he called an "open hearing" on the question. To the latter's amazement, Douglas "quite vehemently" sided with the majority and engaged Frankfurter in an acrimonious debate over *Mooney*, which indicated—at least as Frankfurter saw it—that Douglas either had misinterpreted Finerty's motion or did not grasp the doctrine of the *Mooney* case. Frankfurter became so agitated by this dialogue with Douglas that he made a longhand transcript on the back of an envelope:

Douglas: "[You've] got to do more than use perjured testimony, [you've] got to manufacture it."

Frankfurter: Oh! no! Oh! no! [The] knowing use of perjured testimony is enough. I know a good deal about *Mooney*."

Even Jackson, who voted against habeas corpus, tried to convince Douglas that Finerty had participated in the *Mooney* case and modeled his claims on that precedent; but Douglas, according to Frankfurter, remained adamant: "He couldn't see . . . that Finerty's pleadings here went to anything that he would call jurisdictional. He was still willing to grant certiorari, but could not see how these allegations could be entertained on habeas corpus."⁷⁹

Douglas's resistance to Finerty's application no doubt surprised Frankfurter because only a year before Douglas and Black had rested a dissent upon the principles of the *Mooney* case. By now, both Frankfurter and Jackson believed that Douglas had contradictory motives in the Rosenberg litigation. On the one hand, he worked to retain his image as a liberal tribune who, when necessary, fought alone on behalf of the oppressed. On the other, he thwarted collective efforts to review the case. "Every time a vote could have been had for a hearing," Jackson complained, according to Frankfurter, "Douglas opposed a hearing in open Court, and only when it was perfectly clear that a particular application would not be granted, did he take a position for granting it."⁸⁰ Douglas had voted against three certiorari petitions and had refused to support both Jackson's recommendation to hear oral arguments for a stay and Finerty's habeas corpus plea; but two days after the Court recessed he acted independently to stay the Rosenbergs' executions and precipitated a dramatic special session of the full Court on June 18 and 19.

⁷⁹ Frankfurter, "Rosenberg Memorandum—Addendum," 1, June 19, 1953, Box 65, File 1, FFPHLS; and "Notes of Meeting with W.O.D.," Box 65, File 2, FFPHLS. For Frankfurter's position on Finerty's application, see *Rosenberg et al. v. United States*, 346 U.S. 282 (1953). For the precedents of the *Mooney* case, see *Mooney v. Holohan*, 294 U.S. 103 (1935).

⁸⁰ *Remington v. United States*, 343 U.S. 907 (1952, Cert. denied); and Frankfurter, "Rosenberg Memorandum—Addendum" (FFPHLS), 4.

This is not to suggest that Douglas acted frivolously when he granted the Rosenbergs a stay of execution on June 17. Farmer and Marshall, whose petitions Kaufman had rejected, presented the justice with a persuasive argument: the Rosenbergs might be put to death illegally because the 1917 Espionage Act, upon which they had been indicted and sentenced, had been superseded by the penalty provisions of the Atomic Energy Act of 1946. Under this statute, a death sentence could be imposed for atomic espionage only if the jury recommended it and if the crime had been committed with the intent to injure the United States. The nation must be secure, Douglas wrote in his opinion, against “the nefarious plans of spies who would destroy us.” But he had serious doubts about the imposition of the death penalty, and he stated that the Rosenbergs “should have an opportunity to litigate that issue” in the district court and the circuit court of appeals.⁸¹ At the same time, Douglas also knew before he issued the stay that his performance did not have the support of either the chief justice or a majority of the Court and that even those who remained sympathetic to the legal arguments he presented neither approved of his methods nor, in some instances, trusted his motives. Legally, his position was strong; politically and morally, however, Douglas now functioned in a vacuum without the support of most of his colleagues.

Although he acted independently on June 17, Douglas did not reach his decision to grant the stay hastily. From late Monday afternoon, June 15, until Wednesday morning, he poured over the briefs and statutes, drafted his opinion, and worried endlessly about the response of the other justices. On Tuesday evening Frankfurter told him that the Atomic Energy issue “seemed . . . [to be] one that should be looked into,” but Frankfurter refused to read Douglas’s opinion. Did Frankfurter know, Douglas asked, how Jackson felt about the matter? Should he consult Jackson and Burton? The chief justice, Douglas informed Frankfurter, believed that the issue of the appropriate penalty under the Atomic Energy Act had already been disposed of in “my published memorandum of November 17, 1952 in which I noted that it was clear . . . we had no power to revise the sentence imposed by a District Judge.” Vinson had also told him, Douglas said, that Farmer, the new attorney, had no standing to litigate the issue and that Douglas should lay the entire problem before the conference. The attorney general, Vinson added, had also urged that the new matter go to conference. “Do . . . what your conscience tells you,” Frankfurter lectured him on Wednesday morning, “not what the Chief Justice tells you. . . . Tete-a-tete conversation cannot settle this. . . . That was all I could tell him.” Black, the only justice who saw the opinion before Douglas issued it, thought that Douglas had written an “enduring document.”⁸²

Frankfurter maintained an icy neutrality in the face of Douglas’s repeated calls for advice and support. The chief justice and Jackson, on the other hand, worked to counteract the impact of the expected stay of execution. According

⁸¹ *Rosenberg v. United States*, 346 U.S. 273 (1953), 321.

⁸² Frankfurter, “Rosenberg Memorandum—Addendum” (FFPHLS), 5–6.

to Frankfurter's account, Jackson indicated that he had no objection to Douglas's entertaining Farmer's motion. But FBI documents suggest that Jackson, contrary to the position he took with Frankfurter and Douglas, and at the same time he was discussing the case with them, arranged a meeting on June 16 with Attorney General Brownell and Chief Justice Vinson at which they talked about the unusual strategy of reconvening the entire Court to overturn the stay: "Jackson felt that the whole theory of listening to Farmer's motion was ridiculous and Douglas should have turned it down. . . . Vinson said that if a stay is granted he will call the full Court into session Thursday morning to vacate it."⁸³ Shortly after Douglas issued the stay, Brownell petitioned the Court to vacate it on the grounds that the penalty clauses of the Atomic Energy Act did not pose a substantial question. Over Black's objections the full Court assembled on June 18 to hear arguments, although at least two members of the Court—Vinson and Jackson—had already decided to reverse the stay even before Douglas's opinion had been published.⁸⁴

During the nearly three hours of oral argument on June 18, lawyers and judges frequently erupted with accusations that had little to do with the two principal legal issues before the Court: (1) did they have the authority to vacate Douglas's stay? and (2) did the apparent conflict between the penalty provisions of the Espionage Act and the Atomic Energy Act present "a substantial question" requiring further study and litigation? Attorneys for the Rosenbergs, who had expected Douglas's stay to remain in force over the summer, bitterly condemned the government's efforts to vacate it. Finerty, in addition, lashed out at Irving Saypol: "There never was a more crooked District Attorney in New York than the one who tried the Rosenbergs," he said. Justices Clark and Jackson questioned the presence of Farmer and Marshall as attorneys for the Rosenbergs, and Jackson wanted to know if Irwin Edelman was the same Edelman convicted for vagrancy in California, a remark which prompted another sharp exchange between Jackson and the defense lawyers. Frankfurter asked both the government and the defense whether, assuming Douglas's stay remained in force, the Rosenberg jury could be reconvened after two years or a reconstituted jury would be necessary. Jackson finally indicated that in his opinion the Supreme Court should dispose of the issues without further delay. "It is a point of law," he stated. "Why shouldn't we stay here and decide it and tell the lower court what to do instead of asking them to tell us?"⁸⁵

During the afternoon of June 18 and the morning of June 19, the justices met

⁸³ Belmont to Ladd, June 17, 1953, "Kaufman Papers." According to Belmont's memorandum, Judge Kaufman was the source of this information. He learned of the Brownell-Jackson-Vinson meeting from James B. Kilsheimer III, the assistant United States attorney in New York, who had helped to prosecute the Rosenbergs. Kaufman, in turn, passed the information along to Tom McAndrews, FBI supervisor in New York.

⁸⁴ Many of the justices had already left Washington when Vinson called them back for the special session. Frankfurter, finally located at Owen Roberts's farm in Pennsylvania, hurried back to Washington and spent the night of July 17 at the home of Joseph Rauh, Jr., his former law clerk and one of the founders of Americans for Democratic Action. Rauh recalls that Frankfurter spend a restless night and had harsh words for Douglas. Interview with Joseph Rauh, Jr., August 12, 1975.

⁸⁵ *New York Times*, June 19, 1953, p. 8.

twice before vacating Douglas's stay in a brief *per curiam* opinion prepared by Justice Reed. Burton's notes of those meetings indicate that the discussions became very heated. Black, for one, denounced the special session and the haste involved in the proceedings. "It was terrible," he said, "to come in and consider this issue as though we had it before us." Frankfurter questioned whether the full Court had authority to vacate a stay which Douglas had issued in chambers. Burton tended to agree on this point. Overruling Douglas, he told the others, would undermine the stay system, for it had never been done before. "Let it take due course," Burton said. "There is a substantial question. That's all we should pass on now." But the others rejected these arguments in favor of deciding the question on the merits. Clark said it was "wrong to hold up [the case] any longer." Reed disputed Frankfurter's contention that the Court could not review the stay; Jackson, Minton, and Vinson all believed that the Atomic Energy Act did not apply to the case. The Court was not deciding hastily, Minton concluded, for "the case has been here many times and these questions should have been raised earlier."⁸⁶

Finally, the justices voted on three separate issues. A motion to uphold Douglas's stay, pending complete proceedings in the lower courts, lost 5-3. Burton joined Black and Douglas in the minority, but, according to Burton's notes, Frankfurter abstained. Four justices (Black, Douglas, Burton, and Frankfurter) then voted to shorten the stay by hearing further arguments and requesting additional briefs on the Atomic Energy Act within three weeks. When that motion failed, the Court voted 6-3 to vacate the stay. Burton, who had expressed strong reservations about such an action, cast his vote with the majority when the first two motions failed. Black, Douglas and Frankfurter dissented. That night, after Eisenhower again rejected a plea for clemency, the Rosenbergs were put to death.⁸⁷

The Court's behavior during the final hours of the Rosenberg case became for Frankfurter one of the most depressing episodes in the entire litigation. In contrast to Minton, Frankfurter thought the Court acted in haste, which seemed to him irresponsible, if not worse. Burton, although inclined to accept the government's position on the merits, gave voice to similar feelings. "We should take the time necessary for a regular case," he said. "This has never been adequately briefed by anybody for us." Before the final votes, Burton assured Frankfurter that if four justices wished to uphold the stay or shorten it for additional arguments, he would provide the fifth vote in either

⁸⁶ Burton, Conference Notes on Douglas's Stay, Box 257, HBPLC. Prior to the Rosenberg case, the full Court had always declined to overturn a stay granted by a single justice. See, for example, *Johnson v. Stevenson*, 335 U.S. 801 (1948) and *Land v. Dollar*, 341 U.S. 737 (1951). Ironically, the majority in the Rosenberg case cited these two decisions as authority for vacating Douglas's stay.

⁸⁷ Burton, Conference Notes on Douglas's Stay (HBPLC). Eisenhower declined clemency less than one hour after the Court vacated Douglas's stay, so that he probably did not have the opportunity to read the closing words in Jackson's concurring opinion: "Vacating this stay is not to be construed as endorsing the wisdom or appropriateness to this case of a death sentence." *Rosenberg v. United States*, 346 U.S. 273 (1953), 292-93. In his statement to the press Eisenhower said that he would not intervene because "the Rosenbergs may have condemned to death tens of millions of innocent people all over the world. The execution of two human beings is a grave matter. But even graver is the thought of the millions of dead whose death may be directly attributable to what these spies have done." *New York Times*, June 20, 1953, p. 1.

case.⁸⁸ In his opinion for the majority, published after the executions, Chief Justice Vinson wrote that they had “deliberated in conference for several hours” before deciding that the questions raised in Douglas’s stay were not substantial and that it should be vacated. On the face of a page proof of Vinson’s opinion, Frankfurter wrote an angry notation:

The fact is that all minds were made up as soon as we left the Bench—indeed, I have no doubt . . . before we met on it! At the conference “several hours” were consumed by the remarks of the nine Justices, in their usual order of seniority. The C.J. [Chief Justice] talked at length, mostly in support of his view that there was “waiver,” whatever point was “raised or raisable”—a point he . . . formally abandon[ed]. . . . Most of the time was consumed by consideration whether [the] result should be announced that afternoon . . . or delayed until next day noon! . . . No discussion of [the] merits.⁸⁹

A month later, Frankfurter gave full vent to his displeasure in a published dissent. He argued that the Atomic Energy statute presented a grave question and that Douglas’s stay should not have been vacated: “Without adequate study there cannot be adequate reflection; without adequate reflection there cannot be adequate discussion. . . . We have not had the basis for reaching conclusions and for supporting them in opinions. Can it be said that there was time to go through the process by which cases are customarily decided here?”⁹⁰

Indeed, the majority opinions—written by Vinson, Jackson, and Clark—betray the absence of adequate study and reflection expected in a decision patched together in a day or two. At the threshold, the Court had to meet the question of its power to vacate Douglas’s stay and the necessity for doing so. Vinson could cite neither a prior judicial decision nor a statute that authorized the Court’s action; in fact, all of the case law pointed in the opposite direction. “So far as I can tell,” Justice Black wrote in dissent, “the Court’s action here is unprecedented.” The Chief Justice finally rested the authority upon the Court’s “responsibility to supervise the administration of criminal justice” or, as one critic of the decision noted, the Court’s “inherent powers to control the actions of its judges.” But that vague doctrine ran against the Court’s general reluctance to claim implied or inherent powers for itself and suggested that Douglas acted irrationally or arbitrarily, a point of view categorically rejected by Vinson because “the stay issued . . . was based . . . on a new claim—a question which had not been considered in any prior proceeding.” Unable to provide a coherent answer to the first question, the majority moved to its corollary: the necessity to vacate the stay. If Douglas’s stay remained in force, Vinson argued, many months of litigation would ensue in the lower courts. This process would subvert the proper administration of the laws, because the reasons behind Douglas’s action “raised no doubts of

⁸⁸ Burton, Conference Notes on Douglas’s Stay; and Frankfurter, “Rosenberg Memorandum—Addendum” (FFPHLS), 6.

⁸⁹ For these handwritten remarks, see *Rosenberg v. United States* [346 U.S. 273 (1953)], page proof; the Chief Justice], Box 65, File 4, FFPHLS.

⁹⁰ *Rosenberg v. United States*, 346 U.S. 273 (1953), 309.

such magnitude as to require further proceedings.” The issues were, in fact, so simple and free from doubt that six members of the Court resolved them in less than forty-eight hours.⁹¹

The majority, Vinson wrote, “did not entertain the serious doubts which Mr. Justice Douglas had,” because in their opinion the Atomic Energy Act of 1946 “did not displace” the Espionage Act. When Congress punished conduct by more than one statute, Clark added, the government might invoke either law. It was a “cardinal principle of construction,” he wrote, that the repeal of one law by another was not favored by implication and that “when there are two acts upon the same subject, the rule is to give effect to both if possible.” Finally, both Clark and Jackson argued that the Atomic Energy Act did not apply to the Rosenberg case in any event, because “the crux of the charge alleged overt acts committed in 1944 and 1945,” before that law had been adopted. The government could not have prosecuted the Rosenbergs under the Atomic Energy Act, Jackson declared, because to do so would have violated the constitutional prohibition against *ex post facto* laws. “Since the Atomic Energy Act thus cannot cover the offenses charged,” Clark concluded, “the alleged inconsistency of its penalty provisions with those of the Espionage Act cannot be sustained.”⁹² On the substantive issue of the relationship between the two laws, the views of Vinson, Jackson, and Clark, and the majority possessed an elegant simplicity that seemed to lay to rest all doubts, but as the dissenters wrote at the time and as legal scholars have noted since, this simplicity had been achieved through the avoidance of several thorny matters.

The provisions of one statute did not repeal by implication those of a later statute, Clark wrote, unless the Court found “positive repugnancy” between the provisions of each. Section 10 (b) (6) of the Atomic Energy Act, he noted, provided that “the applicable provisions of other laws shall not be excluded.” Congress intended, he argued, to preserve with “undiminished force” the penalty provisions of the Espionage Act. But Clark’s conclusion did not rest upon an analysis of the legislative history of the 1946 law, which indicated that a major reason for its adoption had been the fear among scientists and legislators that the draconian penalties of the 1917 law inhibited research and the exchange of scientific information.⁹³ Section 10 (b) (6), moreover, referred only to applicable provisions of other laws, not to *all* provisions of existing statutes. The Court did not consider that Congress in 1946 may have intended a partial repeal to the Espionage Act. Furthermore, the Court had long held as a “cardinal principle of construction” that a law adopted with the purpose

⁹¹ *Ibid.*, 208, 287–88; and “The Rosenberg Case: Some Reflections on Federal Criminal Law,” 247–48.

⁹² *Rosenberg v. United States*, 346 U.S. 273, 286, 294–95, 290, 296.

⁹³ As Frankfurter noted in his dissent, there “was not time within twelve waking hours to dig out, to assess, to assemble, and to formulate the meaning of legislative materials,” but he cited the conclusion of James R. Newman, who had been chief counsel for the Senate committee which drafted the Atomic Energy Act in 1946: “It is reasonable to suppose that Congress did not intend to give the prosecuting attorney the option of moving under the Espionage Act instead of the Atomic Energy Act where an offense involving information relating to atomic energy is specifically described in the latter and only broadly and generically encompassed by the former.” *Rosenberg v. United States*, 346 U.S. 273, 307–08.

of regulating a particular phase of a general subject repealed an earlier statute dealing with the entire subject. As precedent for that principle the majority could have cited Justice Jackson in *Massachusetts v. United States*: “. . . this Court has rarely hesitated to interpret the old and general statutes as yielding to the newer and specific statutory scheme.” This rule might have been applied to the Rosenberg case, because the 1917 law punished all types of espionage, while the 1946 statute, which was “newer and specific,” covered only atomic espionage. Finally, Jackson’s and Clark’s *ex post facto* rhetoric only served to obscure further the relationship between the two laws. Since 1798 the Court had held that “an *ex post facto* law is one which imposes a punishment for an act that was not punishable when it was committed or imposes additional punishment.”⁹⁴ The Rosenbergs’ conspiracy, begun in 1944, was punishable at the time of its inception by the Espionage Act. The Atomic Energy Act did not create a new offense or make punishable in 1946 what had not been punishable before 1946. Nor did the “newer and specific” law impose “additional” penalties upon those who committed espionage.

The decisive issue—as Black, Frankfurter, and Douglas pointed out—was that the Rosenbergs had been charged with a conspiracy to commit espionage, including atomic and nonatomic subjects, from 1944 to 1950. No question would have arisen had they been charged only with nonatomic espionage or with a conspiracy lasting from 1944 to 1946, before the Atomic Energy Act came into force. But the Rosenbergs’ conspiracy, Frankfurter wrote, “is one falling in part within the terms of the Atomic Energy Act,” and he doubted that they could be executed for that conspiracy under the Espionage Act, especially in view of the sentencing provisions adopted by Congress in the “newer and specific” law. “Congress does not have to say in so many words that hereafter a judge cannot without jury recommendation impose a sentence of death on a charge of conspiracy that falls within the Atomic Energy Act,” Frankfurter concluded. “It is enough if in fact Congress has provided that hereafter such a death sentence is to depend on the will of the jury.” Before the final arguments Douglas wrote, “I knew only that the question was serious and substantial. Now I am sure of the answer. I know deep in my heart that I am right on the law.” It is difficult, moreover, to refute the harsh conclusion of at least one legal scholar that “in this last stage of an extraordinarily protracted litigation, the rights of the Rosenbergs did not receive the precise and extensive consideration that must characterize the administration of the criminal law.”⁹⁵

FROM THE PERSPECTIVE OF MORE THAN TWENTY YEARS, IT SEEMS VERY DOUBTFUL that the Vinson Court would have saved the Rosenbergs even had the case

⁹⁴ *Massachusetts v. United States*, 333 U.S. 611 (1948), 639; and *Calder v. Bull*, 3 Dall. 386 (1798), 390.

⁹⁵ *Rosenberg v. United States*, 346 U.S. 273 (1953), 304–07, 313; and “The Rosenberg Case: Some Reflections on Federal Criminal Law,” 260. Also see “The Rosenberg Case: A Problem of Statutory Construction,” 754–59.

been fully argued on four occasions. Since 1946 at least five of its members and sometimes seven had displayed a growing reluctance to include Communists or Communist "sympathizers" within the protections of the Constitution and the Bill of Rights. Members of the Communist party and other groups deemed subversive had been denied benefits under the nation's labor laws, jailed for preaching about revolution, deported from the country, and imprisoned without indictment or trial, all in the name of national security and judicial deference to policy choices made by Congress and the executive branch.⁹⁶ Nor was the Court alone in its capitulation to the Red Scare during the early 1950s. Both the Truman and Eisenhower administrations kept public anxiety about Soviet Communism at a high level with strident rhetoric about Red conspiracies abroad and disloyalty at home. In the summer of 1954, Senate liberals, led by Hubert Humphrey, proposed legislation making membership in the Communist party a crime, subject to fine and imprisonment.⁹⁷ It is little wonder that two people, convicted of stealing atomic secrets for the Russians, "did not receive the precise and extensive consideration that must characterize the administration of the criminal law."

Although Douglas's final gamble failed, it seems very likely that had his stay remained in force until lower courts disposed of the issues raised by the Atomic Energy Act or until the Supreme Court reconvened in the fall of 1953, the Rosenbergs might not have been executed. The climate of the Cold War shifted radically in the months following their deaths. On July 28, after three years and 25,000 American casualties, the guns finally fell silent in Korea. Two days after the official armistice, Senator Ralph Flanders of Vermont introduced Senate Resolution 301 calling for the censure of McCarthy and on September 27 a special Senate committee recommended the latter's condemnation for "contemptuous, contumacious, and denunciatory" behavior. More significantly, on September 8 Fred Vinson died and within five months Earl Warren had been sworn in as the new Chief Justice. Although the might-have-beens of history are impossible to calculate, it is difficult to imagine that the Court which toppled racial segregation on May 17, 1954 would have also condemned the Rosenbergs. "The question that faced the Justices," noted Alexander Bickel, one of Frankfurter's law clerks during the final Vinson term, "was whether meeting the latest schedule set for the Rosenbergs' execution was a more important objective than allowing time for the deliberate resolution of difficult legal problems of first impression. The Vinson Court met the schedule with a few hours to spare. . . ."⁹⁸

On the Vinson Court only Black and Frankfurter voted consistently to review the case. At most, three or perhaps four justices might have voted to

⁹⁶ In addition to the examples in notes 38 and 43 above, see *Carlson v. Landon*, 342 U.S. 524 (1952), which upheld the deportation of a mother of three American-born children, who had been a member of the Communist party from 1919 to 1936; and *Shaughnessy v. U.S. ex rel Mezei*, 345 U.S. 206 (1953), which upheld the discretionary imprisonment of an alien without indictment or trial on the grounds that he was a danger to national security.

⁹⁷ Fried, *Men Against McCarthy*, 301-03; and Mary S. McAuliffe, "Liberals and the Communist Control Act of 1954," *JAH*, 63 (1976): 351-67.

⁹⁸ Bickel, *The Supreme Court and the Idea of Progress* (New York, 1970), 5.

reverse the convictions on several issues, but they did not form a coherent bloc of sentiment. Black, for example, was the only justice apart from Burton to express reservations about their convictions in light of the treason clause. Although Burton expressed a willingness to give the couple a hearing, he also indicated that on the merits of each petition he supported the government's position. Four justices—Black, Frankfurter, Douglas, and Jackson—condemned the Saypol incident, but only the first three believed it to be serious enough to warrant review after the circuit court's decision. And they remained the only three who wished to uphold Douglas's stay of execution. In addition to the question of statutory authority, Frankfurter would probably have granted the Rosenbergs a new trial on the basis of Saypol's behavior during the Perl indictment. "Saypol's conduct as set forth in Swan's opinion," Frankfurter told Burlingham, "could not but leave me with disquietude and the Government never contested the allegations against Saypol which Swan condemned. I could say much more—but the rest is silence."⁹⁹

In the margins of one memorandum on the Rosenberg case, Frankfurter made two significant notations: one to his dissenting opinion in *Fisher v. United States*, a federal murder case decided in 1946; the other to section 457 of the American Law Institute's model criminal code, which suggests that courts review "the evidence to determine if the interests of justice require a new trial." His reference to *Fisher* is important in view of Swan's opinion for the circuit court on the Saypol incident. Because the Rosenbergs' lawyers failed to move for a mistrial at the time of the prosecutor's statements to the press, Swan argued, they could not raise this issue on appeal for the purpose of securing a new trial. The *Fisher* case presented a striking analogy. In *Fisher* the Supreme Court affirmed the death sentence of a black janitor, over defense objections that the trial judge failed to instruct the jury on the elements of premeditation required for conviction. Lawyers for the accused had failed to enter a protest during the trial against the judge's error and therefore, wrote Justice Reed for the majority, *Fisher* had waived this issue for purposes of appeal in the circuit court and the Supreme Court. In a sharply worded dissent, Frankfurter called upon the Court to reject "strangling technicalities," especially in capital cases. *Fisher* ought not to die, he argued, because of mistakes made by his attorneys and the trial judge or "because this Court thinks that conflicting legal conclusions of an abstract nature seem to have been 'nicely balanced' by the Court of Appeals."¹⁰⁰ The failure of the Rosenbergs' lawyers to move for a mistrial may not have seemed to him the decisive issue, but only a "strangling technicality." Did not the trial judge, Kaufman, have an obligation to declare a mistrial when confronted with Saypol's conduct? Could Kaufman or the Rosenbergs' attorneys, through either carelessness or indifference, somehow "waive" the rights of criminal defendants on trial for their lives?

⁹⁹ Frankfurter to Burlingham, June 24, 1953, Box 36, FFPLC.

¹⁰⁰ *Fisher v. United States*, 328 U.S. 463 (1946), 476–77, 489. Frankfurter's notations on *Fisher* and section 457 can be found on his Rosenberg opinion, November 17, 1952, in bound volumes for the October term, 1952. FFPHLS.

Why engage in this reconstruction and analysis when the outcome for the Rosenbergs seems so inevitable? Perhaps the answer is found in Frankfurter's last dissent: "To be writing an opinion in a case affecting two lives after the curtain has been rung down on them," he said, "has the appearance of pathetic futility. But history also has its claims. This case is an incident in the long and unending effort to develop and enforce justice according to law. The progress in that struggle surely depends on searching analysis of the past, though the past cannot be recalled, as illumination for the future." For, he continued, "Only by sturdy self-examination and self-criticism can the necessary habits for detached and wise judgment be established and fortified so as to become effective when the judicial process is again subjected to stress and strain."¹⁰¹ Time and again he lectured his colleagues on their own fallibility as men and judges. Culture, experience, and prejudice, he argued, so individualized and fragmented reality that one should not presume to have a monopoly upon truth, virtue, or desirable public policy. Skeptical of absolutes, he articulated a gospel of relativism that encouraged judicial modesty in certain areas, but also made him very sensitive to human failings throughout the legal order. In the case of the Rosenbergs, these convictions served him well, because they suggested the terrible possibility of judicially sanctioned death through error, bias, or deceit that would return to haunt the Supreme Court and the American system of justice in the years ahead.

¹⁰¹ *Rosenberg v. United States*, 346 U.S. 273 (1953), 310.

Propaganda in the Revolution of 1688–89

LOIS G. SCHWOERER

DURING THE ENGLISH REVOLUTION OF 1688–89 PRINCE WILLIAM OF ORANGE AND his adherents in England and Holland carried on an intensive propaganda campaign. They shaped the public image of the prince, interpreted his purposes and policies, and presented events in ways favorable to his interests, while at the same time they blackened the character and policies of King James II. Printed tracts, broadsides, prints, and commemorative medals were the principal devices used to mold the opinion of a broad spectrum of society. Never before in England or on the Continent had these instruments been utilized together in such large number for a single purpose or employed, perhaps, with greater effect. Although propaganda has been studied for other years of the Tudor-Stuart period, the nature and use of propaganda in the Revolution of 1688–89 have not been systematically treated.¹ Such a study invites adjustments of traditional interpretations at a time of renewed interest in the Glorious Revolution.²

An earlier version of this paper was presented under a different title on February 23, 1976 as part of the Folger Shakespeare Library lecture series for 1975–76. The paper grew out of a larger study, of the Bill of Rights, 1689, for which I held a Senior Fellowship from the National Endowment for the Humanities in 1975. Isabel W. Kenrick, a personal friend in London, ably assisted in some aspects of research, while Jop Spiekerman of the University of Leiden and Margaretha Arlman aided in translating from the Dutch. The staffs of many libraries were unfailingly helpful: the Folger Shakespeare Library, the Print Room and the Numismatic Room of the British Museum, the Henry E. Huntington Library, the British Library, Nottingham University Library, the Bodleian Library, the Guildhall Library, and the Carl H. Pforzheimer Library. Her Majesty the Queen of the Netherlands kindly gave me permission to use the collection at the Koninklijk Huisarchief. Richard DeMolen, Henry Horwitz, Joseph W. Martin, and Barbara B. Taft read a version of this article and offered useful suggestions. For all of this assistance and encouragement, I wish to express my thanks. And I wish to express my gratitude to the Trustees of the British Museum, by whose courtesy the illustrative material has been reproduced.

¹J. H. Plumb has remarked that an investigation of the development of political propaganda over the seventeenth century is needed; see "The Growth of the Electorate in England, 1660–1715," *Past & Present*, #45 (1969): 104, n. 37. Two recent books touch upon the issue: John Carswell, *The Descent on England: A Study of the English Revolution of 1688 and Its European Background* (London, 1969), 128, 165, 179 and n., 183, 191, 197, 234; and J. R. Jones, *The Revolution of 1688 in England* (London, 1972), 231–34, 246–49.

²The latest general studies are Jones, *Revolution of 1688*, and Stuart Prall, *The Bloodless Revolution: England, 1688* (New York, 1972). The most recent monograph is David Hosford, *Nottingham, Nobles, and the North: Aspects of the Revolution of 1688* (Hamden, Conn., 1976). Among recent articles are Robert Frankle, "The Formulation of the Declaration of Rights," *Historical Journal*, 17 (1974): 265–79; Henry Horwitz, "Parliament and the Glorious Revolution," *Bulletin of the Institute of Historical Research* (hereafter *BIHR*), 47 (1974): 36–52; J. P. Kenyon, "The Revolution of 1689: Resistance and Contract," *Historical Perspectives: Studies in English Thought and Society in Honour of J. H. Plumb*, ed. Neil McKendrick (London, 1974), 43–70; and Lois G. Schwoerer, "A Journall of the Convention at Westminster begun the 22 of January 1688/9," *BIHR*, 49 (1976): 242–63.

Although it has a pejorative connotation, the word “propaganda” actually means “any systematic scheme or concerted effort for the propagation of a particular doctrine or point of view.” The effort in 1688–89 included time-honored methods of spreading news and views—sermons,³ poems and odes,⁴ firework displays, and ceremonies.⁵ They too deserve further study, but this essay will focus on the more innovative and important devices—printed tracts, pictures, and medals. The tracts and pamphlets have been dismissed as “not very interesting” on the grounds that they are “theoretically . . . defective.”⁶ But they do provide insight into the political ideas circulated at the time. Some of the prints and medals, many of which are to be found in the British Museum Print Room and Numismatic Room, were reproduced with appreciative comment by early historians, but modern writers have used them as decorations for their texts rather than as sources. Students of other eras and other disciplines have demonstrated how these unconventional and ephemeral materials may enliven one’s understanding of the past.⁷ These same materials can enlarge and vivify our comprehension of the Revolution of 1688–89.

³ A study of preachers and their sermons in London and elsewhere during the Revolution of 1688–89 would be worth doing. The number of sermons preached must be enormous. See a useful collection of thirty-one sermons bound together by a contemporary and inscribed “A Collection of Sermons in the Revolution time, and the first year of King William and Queen Mary, 1688, 1689,” at the Folger Shakespeare Library (hereafter FSL), S 3348. The kind of study suggested for 1688–89 has been done for the Civil War, when sermons were preached before members of the House of Commons. See, for example, Godfrey Davies, “English Political Sermons,” *Huntington Library Quarterly*, 3 (1939): 1–22; E. W. Kirby, “Sermons before the Commons, 1640–42,” *AHR*, 44 (1939): 528–48; and H. R. Trevor-Roper, “The Fast Sermons to the Long Parliament,” *Essays in British History Presented to Sir Keith Feiling*, ed. H. R. Trevor-Roper (London, 1964), 85–138. The Fast sermons are available in thirty-four volumes, reprinted in 1970–71. Also see William Haller, *The Rise of Puritanism: or, The Way to the New Jerusalem as Set Forth in Pulpit and Press from Thomas Cartwright to John Lilburne and John Milton, 1570–1643* (New York, 1938), and Christopher Hill, *Society and Puritanism in Pre-Revolutionary England* (London, 1964), esp. 30–78.

⁴ Poems and odes in abundance memorialized almost every event of the Revolution. Some of these verses are reprinted in George deF. Lord, ed., *Poems on Affairs of State: Augustan Satirical Verse, 1660–1714*, 7 vols. (London, 1963–75). There is a large collection of manuscript poems and odes at the Henry E. Huntington Library and the Nottingham University Library. C. V. Wedgwood, *Poetry and Politics under the Stuarts* (Cambridge, 1960), 186–90, mentions the poems written in 1688–89, but neither they nor the street songs have been systematically treated. Thomas Wharton, the author of “Lilliburlero,” claimed that he had sung King James II out of three kingdoms; for “Lilliburlero,” see C. M. Simpson, *The British Broadside Ballad and Its Music* (New Brunswick, N.J., 1966), 449–55. Further, it is worth noting that the attribution of the “Rock-a-Bye Baby” nursery rhyme to the events of 1688–89 (put forward by Katherine E. Thomas, *The Real Personages of Mother Goose* [New York, 1930], 288–90) is regarded as “purely speculative” by Peter Opie, editor of *The Oxford Dictionary of Nursery Rhymes* (Oxford, 1951); letter of August 13, 1974 to author. It is of interest, however, that the “Rock-a-Bye Baby” tune is a variation of the one to which “Lilliburlero” is sung.

⁵ For example, William of Orange’s arrival at Exeter, his arrival in London, the procession to and the ceremony in the Banqueting Hall in mid-February, and the coronation in April—their preparation, actual execution, and representation—would be worth studying.

⁶ Jones, *Revolution of 1688*, 317. Jones deals with pamphlets published earlier in 1688 and briefly with William’s manifesto.

⁷ Nicholas Chevalier, in his *Histoire de Guillaume III. Roy d’Angleterre, d’ecosse de France, et d’Irlande, Prince d’Orange, etc. Contenant ses Actions les plu memorables, depuis sa Naissance jusques à son elevation sur le Trone, & ce qui s’est passe depuis jusques à l’entire Reduction du Royaume d’Irlande. Par medailles, inscriptions, arcs de triomphe, & autres monumens publics* (Amsterdam, 1692), published “with privilege,” based his account largely on such memorabilia. Nicholas Tindal reproduced about 400 medals in *The Mettalick History of the Reigns of King William III and Queen Mary, Queen Anne, and King George I* (London, 1747), volume 3 of his edition, which he translated from the French, of Paul Rapin de Thoyras, *The History of England*; Tindal commented on the medals in the preface (iii, iv). Walter Harris, *History of the Life and Reign of William-Henry, Prince of Nassau and Orange, Stadtholder of the United Provinces, King of England, Scotland, France, and Ireland, etc.* (Dublin, 1749), reproduced 49 medals; and C. M. Firth illustrated his 1913 edition of Lord Macaulay’s *History of England* with medals and commented upon the value of the iconographic material in the preface. Twentieth-century historians,

IF WILLIAM'S PURPOSES WERE TO BE FULFILLED WITHOUT SIGNIFICANT BLOOD-shed, he needed a major effort to influence public opinion. Prince William of Orange, who invaded England in November 1688, had no direct claim to the English throne, but he did have a place in the line of succession. Born in 1650 to Mary Stuart, the sister of King James II, and posthumously to William II of Orange, this present William was married to another Mary Stuart—the Protestant daughter of James II and his first wife Anne Hyde—and was thus both James's nephew and his son-in-law. A Dutch prince, the stadholder of the Dutch republic since 1672, and a Calvinist, William became the principal opponent on the Continent of the Sun King, Louis XIV of France. Long before the Revolution of 1688–89 he had been a careful observer of England's internal and foreign policies. While maintaining friendly relations with his royal relatives, he formed close ties with their opposition to bring England into an alliance against France and protect the dynastic interests of his wife and himself. Over the years his personal popularity with the English public grew, as did his political options. During the Exclusion Crisis of 1678–83, when the "first Whigs" tried to bar James (then Duke of York and an avowed Catholic) from the succession to the throne, William shrewdly resisted the efforts of some politicians to draw him deeply into the controversy and use him to their own advantage.⁸

In 1685 James became king of England and almost immediately alienated a substantial portion of the politically conscious nation, Whigs and Tories alike. Whatever James's ultimate intentions, the steps he took suggested to many people that he aimed to Catholicize the nation, destroy Parliament, weaken local government, and create a centralized government backed by a standing army and allied to Catholic France. Disaffected Englishmen, many of whom had opposed James earlier, looked again to William to assist them in redressing grievances against James. Although the nature and timing of William's role in the preliminaries to the Revolution have been disputed, it is clear that in the spring of 1688 William and these English conspirators agreed that the prince must come with a force within the year. In June a baby boy was born to James and his second wife, Mary of Modena. Thus James's son, James Francis, displaced William's wife Mary in the line of succession to the English throne and made William's own claim remoter still. Three weeks after the birth seven major English conspirators sent a letter, written at William's insistence, to the Dutch prince. The letter invited William to come to England

however, have largely ignored these materials, leaving it to popular writers—John Miller, in his *The Life and Times of William and Mary* (London, 1974), and Henri and Barbara van der Zee, in their *William and Mary* (London, 1973)—to use such graphic materials as illustrations. Yet several recent studies demonstrate what can be done: Herbert Atherton, *Political Prints in the Age of Hogarth* (Oxford, 1974); and M. Dorothy George, *English Political Caricature to 1792: A Study of Opinion and Propaganda* (Oxford, 1959), and *Hogarth to Cruickshank: Social Change in Graphic Satire* (New York, 1967). Also see John Brewer, "The Faces of Lord Bute: A Visual Contribution to Anglo-American Political Ideology," *Perspectives in American History*, ed. Donald Fleming and Bernard Bailyn, 6 (1972): 95–116; and Jean H. Hagstrum, "Verbal and Visual Caricature in the Age of Dryden, Swift, and Pope," *England in the Restoration and Early Eighteenth Century* (Los Angeles, 1972), 173–95.

⁸ See Stephen Baxter, *William III* (London, 1966), 160–77, and J. R. Jones, *The First Whigs: The Politics of the Exclusion Crisis, 1678–1683* (London, 1961), 83, 85, 90, 128–30, 136, 140, 151, 194–96, for William's role.

with a force and promised him support but, significantly, said nothing about the final solution to the crisis nor about William's or James's future role in England's government. That was left quite open.⁹

On November 5, 1688, his arrival anticipated by widespread enthusiasm, William landed with an army at Torbay.¹⁰ Subsequent events, however, were neither inevitable nor smooth. Three and a half months of mounting tension, division, some fighting, and much political maneuvering ensued. Around the country scattered violence occurred almost immediately, while in London the mob rioted in mid-December.¹¹ James, his nerves shattered (as persistent nosebleeds testified), but with the calculation that his flight would throw England into such confusion that he would be recalled, took refuge in late December with his wife and baby son at the court of Louis XIV. After their departure, in conformity with the expressed desires of Englishmen, a Convention Parliament was elected to resolve the crisis.¹² In the weeks prior to its meeting, no unanimity of opinion about settling the government emerged. On the contrary, men aired alternative proposals in "frequent consults and caballs," and predictions about the outcome multiplied.¹³ The possibility of recalling James was not ignored; the "common letters" noted in mid-January that "at or before the Convention the King will be addressed so to return."¹⁴

⁹ See Baxter, *William III*, 223–34, for the most coherent and persuasive analysis. Also see Jones, *Revolution of 1688*, chap. 8 and 250–53, 280–81, 285–86; and Lucile Pinkham, *William III and the Respectable Revolution: The Part Played by William of Orange in the Revolution of 1688* (Cambridge, Mass., 1954), *passim*. Baxter identifies the conspirators involved; *William III*, 230.

¹⁰ A contemporary noted on October 20 that the invasion "may be called the Merry Invasion," so widespread was the approval of it; see William Westby, "A Continuation of my Memoires or Memoranda Book Jan. 1687/8–Jan. 1689," FSL, V.a 350, 40. Other notices of public approval at this time include Russell J. Kerr and Ida Coffin Duncan, eds., *The Portledge Papers, Being Extracts from the Letters of Richard Laphorne, Gent. of Hatton Garden, London, to Richard Coffin, esq., of Portledge, Bideford, Devon* (London, 1928), 47, 48; John Evelyn, *The Diary of John Evelyn*, ed. E. S. deBeer, 4 (London, 1955): 600; and Sir John Reresby, *Memoirs of Sir John Reresby*, ed. Andrew Browning (London, 1936), 522.

¹¹ Bodleian Library (hereafter Bodl.), Carte MSS, 130, f. 303; British Library (hereafter BL), Add. MSS, 29,594, f. 131, Add. MSS, 34,487, f. 35, and Add. MSS, 34,510, 4:241–42; Westby, "A Continuation of my Memoires or Memoranda Book," 45, 47; "The Newdigate Newsletters, Addressed to Sir Richard Newdigate, 1st Bart., and to 2nd Bart., 1673/74–1715," FSL, LC, 1934, 1944; G. I. W. Agar-Ellis, Lord Dover, ed., *The Ellis Correspondence: Letters Written during the Years 1686, 1687, 1688 and Addressed to John Ellis*, 2 (London, 1831): 290–93; and Kerr and Duncan, *The Portledge Papers*, 50. For a study of the uprising of the mob, see W. L. Sachse, "The Mob and the Revolution of 1688," *Journal of British Studies* (hereafter *JBS*), 4 (1964): 23–40.

¹² James's first and unsuccessful flight was December 11, and the second, December 23; his wife and son had gone on December 10. Englishmen of "all ranks and conditions" wanted a parliament called to settle the crisis, and from October through December this proposal dominated discussions with James and William; for a brief account, see Horwitz, "Parliament and the Glorious Revolutions," 36–39. For the election, see J. H. Plumb, "The Elections to the Convention Parliament of 1689," *Cambridge Historical Journal*, 5 (1937): 235–55; Horwitz, "Parliament and the Glorious Revolution," 40–42; and Alan Simpson, "The Convention Parliament of 1688–89" (D.Phil. thesis, Oxford University, 1939), 14–36.

¹³ Narcissus Luttrell, *A Brief Historical Relation of State Affairs from September 1678 to April 1714*, 1 (Oxford, 1857): 497; BL, Add. MSS, 40,621, f. 5; Roger Morrice, "Entr'ing Book, Being an Historical Register of Occurrences from April, Anno 1677 to April 1691," FSL (photocopy), Q, 422, 424 (the original is in Dr. Williams's Library in London); and National Library of Wales, Aberystwyth, Ottley Papers, 469(1) (Dr. Clyve Jones called my attention to this manuscript).

¹⁴ Morrice, "Entr'ing Book," 427. Also see "The Newdigate Newsletters," LC 1964; Historical Manuscripts Commission (hereafter HMC), *The Manuscripts of His Grace, the Duke of Portland*, 3: 421; S. W. Singer, ed., *Correspondence of Henry Hyde, Earl of Clarendon, and of His Brother, Laurence Hyde, Earl of Rochester*, 2 (London, 1828): 238, 244, 246; "Journaal van Constantyn Huygens, den zoon, van 21 October 1688 tot 2 September 1696," *Werken Uitgegeven door het Historisch Genootschap* (Utrecht, 1876–78), n.s., 23: 60, 67. For one

Among James's former soldiers, grumblings, desertions, and near mutiny expressed dissatisfaction with the turn of events. William's life was threatened, one of his Dutch guards was found murdered, and private duels provoked by political disagreement were fought.¹⁵ "Some persons," it was reported, "make it their endeavour to sow jealousies and foment divisions among us."¹⁶ So tense became the situation after the Convention opened on January 22 that bloodshed was feared.¹⁷ William was fully aware of the differences of opinion in the Convention and sensitive enough to ask a Dutch emissary in London not to report them in his letters to Holland.¹⁸

If the prince and his friends were to achieve the end of making him king of England, which—whatever his earlier intention—was indisputably his aim after James's flight, and were to accomplish this goal without violence, a broad consensus in his favor was essential. To achieve this consensus an intensive campaign of propaganda was carried on throughout the months of the Revolution. Partly because of the success of his propaganda effort, a solution to the crisis of 1688–89 satisfactory to William was agreed upon. On February 13 in a unique ceremony, William and Mary, Prince and Princess of Orange, were proclaimed king and queen of England with the sole administrative authority vested in William alone. This ceremony brought one phase of the Revolution of 1688–89 to a close. It also brought a new set of problems for King William III, some of which were created by the very effort used to persuade the public to accept the solution which had finally been negotiated.

THE REVOLUTION OF 1688–89 WAS NOT THE FIRST TIME THAT PRINTED TRACTS, prints, and medals had been used to influence politics.¹⁹ Since the Reformation the English crown and its counselors had used printed tracts to shape public

aspect of the work of the loyalists, see Robert Beddard, "The Guildhall Declaration of 11 December 1688 and the Counter-Revolution of the Loyalists," *Historical Journal*, 11 (1968): 403–20; also see Horwitz, "Parliament and the Glorious Revolution," 36–38.

¹⁵ Evelyn, *Diary*, 612; Singer, *Correspondence of . . . Clarendon*, 231, 234; Luttrell, *Brief Historical Relation*, 496, 505; Morrice, "Entr'ing Book," 366, 397, 399, 407, 432, 433, 435, 449, 455; Dover, *Ellis Correspondence: Letters Written during the Years 1686, 1687, 1688*, 374; the *London Courant*, December 18–22, 1688, #4; and the *London Mercury*, December 13–18, 1688, #2.

¹⁶ Luttrell, *Brief Historical Relation*, 496.

¹⁷ Morrice, "Entr'ing Book," 452, 454; and BL, Add. MSS, 40.621, f. 12. Also see "the truly great discontents" reported by Reresby, *Memoirs*, 553; and Singer, *Correspondence of . . . Clarendon*, 257; Gilbert Burnet, *Bishop Burnet's History of His Own Time, with Notes by the Earls of Dartmouth and Hardwicke, Speaker Onslow, and Dean Swift* (Oxford, 1833), 3: 398n., 405n. William's life was again threatened; see Bodl., Rawlinson MSS, D, 1079, f. 5.

¹⁸ J. G. van Terveen, ed., "Uittreksels uit het Bijzonder Verbaal Nopens de Deputatie en Ambassade Daarop Gevolgd in Engeland, 1689, Gehouden Door Mr. Nicolass Witsen, Burgemeester te Amsterdam." *Geschied-en Letterkundig Mengelwerk van Mr. Jacobus Schellema* (Utrecht, 1823), Derde Deel, pt. 2, 139.

¹⁹ The impact of printing in early modern Europe, largely on elite groups, has been explored by Elizabeth L. Eisenstein in "Some Conjectures about the Impact of Printing on Western Society and Thought: A Preliminary Report," *Journal of Modern History*, 40 (1968): 1–56, and "The Advent of Printing and the Problem of the Renaissance," *Past & Present*, #45 (1969): 19–89. For the connections between printing and popular culture in sixteenth-century France, see Natalie Z. Davis, *Society and Culture in Early Modern France* (Stanford, Calif., 1975), esp. 189–226. Also see Joseph Klaitz, *Printed Propaganda under Louis XIV: Absolute Monarchy and Public Opinion* (Princeton, 1976); and Joseph Jacquiot, *Médailles et jetons de Louis XIV, d'après le manuscrit de Londres* (Kensieck, 1970).

opinion, and people outside the government had done likewise. During the seventeenth century, pamphlet warfare was part of every major political crisis.²⁰ Nor was this the first time that William had used tracts to promote his political interests. In the early 1670s, for example, William employed Peter Du Moulin, who wrote a pamphlet largely credited with turning the Commons against the French alliance; and, as the historian of that episode has commented, the prince used the same technique then that he would apply in the fall of 1688.²¹ Even in 1687 and early 1688 William relied upon the press; Gaspel Fagel's printed letter presenting William's views on the repeal of the Test Acts was designed to undermine James. Before William landed, his agents advised him on the content and distribution of tracts and established connections with printers and publishers in England. One agent wrote William in April 1688 that if he wanted to keep England "in humor [he] . . . must entertain it by papers."²² Prints and medals, moreover, had also been used before 1688 by Englishmen and, more especially, by Dutchmen to convey a polemical message.²³ Ever since their revolt from Spain in the sixteenth century, the Dutch had been the "chief source of pictorial propaganda."²⁴ In the late 1660s and early 1670s they employed vicious iconographic material to satirize English individuals and policies. With less apparent enthusiasm Englishmen had also used prints and medals to comment on politics. Those who attempted to exclude James, Duke of York, for one example, did so by pictures as well as by printed tracts.²⁵ In 1688, then, not only was William an experienced publicist, but the English themselves had also become accustomed to the public airing of political views in written and visual form.

²⁰ See G. R. Elton, *Policy and Police: The Enforcement of the Reformation in the Age of Thomas Cromwell* (Cambridge, 1972), chap. 4; Cromwell's campaign was the first time any government had employed printed propaganda (206–07). Also see William Haller, *Foxe's Book of Martyrs and the Elect Nation* (London, 1963); and Jennifer Loach, "Pamphlets and Politics, 1553–8," *BIHR*, 48 (1975): 31–44. For some later examples, see Carolyn A. Edie, "The Popular Idea of Monarchy on the Eve of the Stuart Restoration," *Huntington Library Quarterly*, 29 (1976): 343–73; O. W. Furley, "The Whig Exclusionists: Pamphlet Literature in the Exclusion Campaign, 1679–81," *Cambridge Historical Journal*, 13 (1957): 19–36; K. H. D. Haley, *William of Orange and the English Opposition, 1672–74* (Oxford, 1953); H. Rusche, "Merlini Anglici: Astrology and Propaganda from 1644 to 1651," *English Historical Review* (hereafter *EHR*), 80 (1965): 322–33, and "Prophecies and Propaganda, 1641 to 1651," *EHR*, 84 (1969): 752–70; Lois G. Schwoerer, "The Fittest Subject for a King's Quarrel: An Essay on the Militia Bill Controversy, 1641–1642," *JBS*, 11 (1971): 45–76, and "No Standing Armies!" *The Antiarmy Ideology in Seventeenth-Century England* (Baltimore, 1974), chap. 8; and W. A. Speck, "Political Propaganda in Augustan England," *Transactions of the Royal Historical Society*, 5th ser., 22 (1972): 17–32.

²¹ Haley, *William of Orange and the English Opposition, 1672–1674*, 52–53, 97–98, 105–07, 111, 222. The pamphlet was England's *Appeal from the Private Cabal at Whitehall to the Great Council of the nation, the Lords and Commons in Parliament assembled* (1673). It was reprinted in 1689 in *State Tracts: Being a Collection of several Treatises Relating to the Government. Privately Printed in the reign of King Charles II.*

²² Nottingham University Library (hereafter NUL), Portland MSS, PwA. 2159; and see PwA. 2099, 2110, 2112, 2118, 2120, 2124, 2126, 2139, 2143. Also see Jones, *Revolution of 1688*, 211, 215, 226–27, and chap. 8: "William's English Connection."

²³ See the *Catalogue of Prints and Drawings in the British Museum, Political and Personal Satires*, prepared under the supervision of F. G. Stephens (London, 1870), vol. 1: 1320 to April 11, 1689. Its title notwithstanding, the volume contains references to medals.

²⁴ George, *English Political Caricature to 1792*, 4 and *passim*.

²⁵ A contemporary said the Dutch prints insulted England so grievously that war against Holland was justified; see George, *English Political Caricature to 1792*, 48–50. Also see B. J. Rahn, "A Ra-Ree Show—A Rare Cartoon: Revolutionary Propaganda in the Treason Trial of Stephen College," *Studies in Change and Revolution: Aspects of English Intellectual History, 1640–1800* (London, 1972), 77–98.

Tracts, prints, and commemorative medals appeared at every important step in the Revolution of 1688–89. As their timing indicates, almost all had the specific purpose of molding public opinion in the prince's favor. A broadside, entitled simply *Character*, was printed at The Hague on October 12, a month before William sailed.²⁶ The date and place of its publication and its flattering terms suggest that someone close to William had a hand in it and perhaps coordinated its appearance with the other tracts known to have been written by William and his close associates. Another pamphlet—*The Character of His Royal Highness William Henry Prince of Orange*—appeared during the meeting of the Convention, when an adulatory account of the prince's personal qualities could do him the most good. Significantly, it bore the imprimatur "With Allowance," indicating his knowledge and approval. And yet another tract touching on William's character was licensed four days before the Convention opened.²⁷ One author candidly declared that his pamphlet was intended to reach a much larger number of people than could have known William personally or learned about him from earlier printed accounts.²⁸ Propaganda specifically aimed at a cosmetic treatment of the stadholder appeared very early in the sequence of events in 1688 and 1689. The major purposes were to magnify William's strengths, to deny or explain away his weaknesses and faults, and to present his proposals in their most favorable light.

First of all, William had to appear kingly to the English public. Yet the prince was not at all attractive physically. Standing just over five and a half feet tall, he had spindly legs and little feet like those of a child. His thin, slight body with its slightly deformed back supported a head marred by a huge, beak-like, and crooked nose that destroyed the symmetry of his face. Black, poorly aligned teeth further detracted from his appearance. One contemporary privately declared that there was nothing beautiful about William but his eyes, an assessment which failed to praise his only other physical asset—his long, thick, and naturally wavy hair.²⁹ The pamphlets, however, referred to the prince's "wonderful proportions and features." His health was as frail as his body, the result of chronic asthma which led to frequent colds and a persistent cough. But the pamphleteers attributed to him a "robust and healthy" constitution. The stadholder, they stated, practiced "exemplary temperance and sobriety" out of principle rather than physical necessity. Other admirers

²⁶ The date was October 12, New Style (N. S.), used on the Continent, or October 2, Old Style, used in England.

²⁷ For evidence of the former's appearance during the Convention, see *Character of His Royal Highness* (London, 1689), 7; the latter—*A Dialogue Between Dick and Tom concerning the present posture of Affairs in England*—was licensed on January 18, 1689.

²⁸ *Character of His Royal Highness*, 4. One such earlier account was that of Sir William Temple: *Observations Upon the United Provinces of the Netherlands*, ed. Sir George Clark (Oxford, 1972), 135. Six editions in English appeared between 1673 and 1693. The passage about William was used verbatim in *The History of the most Illustrious William, Prince of Orange; Deduc'd from the First Founder of the Antient House of Nassau, Together with the most Considerable Actions of this Present Prince* (London, 1688), 189–90, and was referred to in *Papish Treaties Not to be Rel'd Upon: In a Letter from a Gentleman at York, to his Friend in the Prince of Orange's Camp. Addressed to all Members of the next Parliament* (London, 1688), 3.

²⁹ F. J. L. Krämer, ed., "Mémoires de Monsieur de B . . . ou Anecdotes, Tant de la cour du Prince d'Orange Guillaume III, que des principaux seigneurs de république de ce temps," *Bijdragen en Mededeelingen van het Historisch Genootschap*, 19 (Utrecht, 1898): 79.

acclaimed William's personal morality and integrity. "Women and Wine, the two great seducers of mankind, never could entice him to the least frailty." So fine an example did he set that his courtiers were like "Professors of ethics" and his court appeared to be "another Athens of Philosophers, the only Seat of Justice and Vertue now left in the World."³⁰

To be kingly, William also had to seem kind and courageous, just and wise. Thus, though the stadholder was known by his intimates to be irritable and short-tempered, tracts called him "benign," even "affable" and "of . . . sweet temper." He conquered his enemies by the "charming dispositions of his Mind," and his nature combined the "meekness of Moses" with the "Courage and Valiancy of Joshua."³¹ Although taciturnity and reserve had long characterized his personality, a pamphleteer explained that the prince "talks not much, but very well," and was able to discern the best advice.³² To complement the image which appeared on medals and in poems, the prince was described as a "young Belgick Lion" (the traditional symbol of Holland).³³ Words failed him, one author averred, when he tried to depict the military prowess and valor displayed by the stadholder on the battlefield. So strong was his character that William was able to maintain "exact discipline" in an army composed only of "well-bred Gentlemen" and "honest Citizens." These accomplishments notwithstanding, the prince, it was said, shunned personal glory and was without ambition.³⁴

In the circumstances of 1688 personal charisma and military prowess were less important, however, than the prince's civil accomplishments. Though some thought William arbitrary and claimed that he had "undone" the Dutch burghers' "liberties,"³⁵ the pamphleteers asserted that the stadholder had never violated the personal rights or abridged the municipal privileges of the Dutch citizenry. "Justice remarkable, prudence inimitable, [and] temperance extraordinary" had characterized William's rule in Holland. That William had revived the office and powers of the stadholder in 1672 should cause no alarm; for the prince had taken that step out of necessity—for the safety, even the continued existence, of the United Provinces. And, although he was said to tolerate Roman Catholics only with difficulty, he was moderate in his

³⁰ *Character of His Royal Highness*, 7; *Popish Treaties Not to be Rely'd Upon*, 3; and *Character*. That William did not drink was noticed by an anonymous observer in June 1688, as quoted in Nesca Robb, *William of Orange: A Personal Portrait*, 2 (London, 1962): 262.

³¹ "Journaal van Constantyn Hugens," 60, 64; van Terveen, "Verbaal . . . Witsen," 140; Burnet, *History of His Own Time*, 4: 2, 561n., 565–66; *Character of His Royal Highness*, 3, 7; and *Character*. Moses was described as a man "very meek above all" in Numbers 12:3.

³² *Character of His Royal Highness*, 7.

³³ The lion as the symbol of the Netherlands was well established. See George, *English Political Caricature to 1792*, 44 and n. 2. For an example of a medal, see page 866 below. *A Congratulatory Poem to His Highness, the Prince of Orange upon His Arrival in London* (London, 1688) contained the couplet, "Methinks I heard the Belgick Lyon roar/Landed in Triumph on the British shoar."

³⁴ *Character of His Royal Highness*, 5, 7; *A Dialogue Between Dick and Tom*, 9; and *Character*.

³⁵ *Some Reflections upon His Highness the Prince of Orange's Declaration*, in *A Collection of Scarce and Valuable Tracts . . . Selected from . . . Public as well as Private Libraries, Particularly That of the Late Lord Somers* (London, 1748–51), 9: 294. It is worth noting that Nicolass Witsen, the mayor of Amsterdam, no admirer of the prince, understood that he had been brought to England to show by his presence unanimity between William and the city of Amsterdam. See van Terveen, "Verbaal . . . Witsen," 137.

own religious views and regarded no man as “factious” unless he disturbed the public peace.³⁶ The implication was clear: compared with the arbitrary James, William was a benevolent ruler, and, in many respects, the epitome of an ideal prince.

The effort to touch up William’s appearance, personality, and past achievements, however, was secondary to the effort to cast the prince’s current purposes and future policies in the most favorable light. Among the many tracts devoted to this latter end, two stand out: the *Declaration of His Highness William Henry, Prince of Orange, of the Reasons Inducing Him to Appear in Armes in the Kingdom of England for Preserving of the Protestant Religion and for Restoring the Lawes and Liberties of England, Scotland, and Ireland* and its postscript, the *Second Declaration of Reasons*. The first, dated October 10, outlined William’s policies and the second, dated two weeks later, provided further explanation of the prince’s intentions; together they were the most important instruments of William’s propaganda effort. The manner of composing the first declaration shows what importance William and his close associates attached to the shaping of public opinion. A first draft of a public statement justifying the invasion was prepared by several of William’s English conspirators and brought to the prince in Holland in August 1688. William, his English and Dutch advisers, and some members of the English colony at The Hague, whose opinion William solicited, all reviewed the draft. As reported by Gilbert Burnet—an important adviser to William at this time and later a bishop—their disputes over certain clauses in the draft reveal that the final text of the *Declaration of Reasons* was dictated by considerations of propaganda and political partisanship.³⁷ For example, some counselors argued that in the manifesto the dispensing power—that is, the discretionary power to set aside laws traditionally exercised by the king—should be made the “main ground” for the expedition, because “the body of the whole nation” had been disturbed by James’s use of it, and, therefore, it “would seem very strange” if it were not the main reason for William’s invasion. Others maintained that the dispensing power was a legal power of the crown which had been exercised for ages and that James’s stretching of that legal power was not a just basis for an invasion. They charged that political calculation and not sincere abhorrence of the use of the dispensing power was the real reason for giving it first priority. The aim, they said, was to win over the bishops and high Tories who had suffered the most as a consequence of the king’s employing the dispensing power.

William’s advisers also disputed the role that the trial of the Seven Bishops should play in justifying William’s decision “to appear in armes in the Kingdom of England.” In an effort to appeal to Anglicans and Tories, some advisers, it was said, insisted that the trial figure prominently in the manifesto. Pointing to the widespread support of the bishops, these unidentified

³⁶ *A Dialogue Between Dick and Tom*, 9; *Popish Treaties Not to be Rely’d Upon*, 4; *Character of His Royal Highness*, 7; and *Character*.

³⁷ See Burnet, *History of His Own Time*, 3: 283–84, 300, 308–10, for the disputes.

advisers maintained that a lack of emphasis on the trial “would be made use of” to persuade people of the prince’s “ill will” toward the nation. Further, inclusion of the clause that people from all walks of life, and not just seven leaders, had invited the prince was credited to an adviser who afterwards confessed to great uneasiness about it.³⁸ Again, although at least one man objected on the grounds that the evidence was inconclusive, others “absolutely insisted” upon including the charge that James’s son was a fraud and the promise that a newly elected Parliament would investigate the baby’s birth. This point reflected the advice of one of William’s agents, James Johnstone, who declared in a memorandum on the subject that it would “do much good in England” to publish suspicions about the baby and to supply the reasons for them. He wrote, “Whoever knows the present dispositions of mens spirit in England must know” that such a thesis should be published to confirm believers and persuade doubters.³⁹ Finally, William himself objected to the first draft: “There is much that needs to be changed,” he wrote to his chief confidant, William Bentinck. “You will see that by the conclusion I am placed entirely at the mercy of Parliament.” He continued, “Handing one’s fate over to them is not without hazard.” As much as he disliked that prospect, William indicated that he did “not think” that it could “be otherwise.”⁴⁰ The remark underscored the prince’s political sensitivities and his willingness to subordinate his private views and to hazard his fate to the demands of propaganda to achieve a more palatable public image. William’s concern that there was danger to him in giving such a large role to Parliament was not misplaced. In some respects the prince was hoisted on the petard of his own propaganda.

The style as well as the content of the *Declaration* was the object of careful attention. Gilbert Burnet shortened and enlivened a draft prepared by one of William’s Dutch advisers. Burnet’s paper, itself interlined and corrected, survives in the Dutch archives.⁴¹ These efforts resulted in one of the most effectively written tracts that appeared in the entire campaign. Readable, and for the most part moderate in tone, it argued William’s case in a clear and detailed manner, but not at such length that it bored the reader. Even the English ambassador at The Hague, who abhorred the contents of the *Declaration*, described its style as “civil and smooth” and predicted that it would “gain the people’s Affections.”⁴²

³⁸ Morrice, “Entr’ing Book,” 383.

³⁹ N. Japikse, ed., *Correspondentie van Willem III en van Hans Willem Bentinck, Ersten Graaf van Portland* (‘s-Gravenhage, 1928), RGP, Kleine Serie, 24: 603; and “Journaal van Constantyn Huygens,” 67. Doubts about the birth surfaced almost immediately, as did vicious satirical verse. Lord., *Poems on Affairs of State*, vol. 4: 1685–1688, ed. Galbraith N. Grump (New Haven, Conn., 1968), 235–72.

⁴⁰ Japikse, *Correspondentie van Willem III en van Hans Willem Bentinck*, 23: 49. William’s misgivings apparently continued in September; see NUL, Portland MSS, PwA, 1659.

⁴¹ Manifest van den Koning Willem III aan de Engelsche Natie, 1688, Oct. 10 (N.S.), Minuut van Gilbert Burnet, Koninklijk Huisarchief, IX, a.15 (old number 2638). Burnet also edited the draft of the *Second Declaration* prepared by Gaspar Fagel. See Japikse, *Correspondentie van Willem III en van Hans Willem Bentinck*, 24: 620–21 and n. 3.

⁴² BL, Add. MSS, 41,816, f. 249; this was a perceptive observation by the ambassador, Ignatius White, Marquis d’Albeville, whose political acuity and personal integrity have been controverted. See E. S. deBeer, “The Marquis of Albeville and His Brothers,” *EHR*, 45 (1930), 397–408. Cf. Jones, *Revolution of 1688*, 217, 257–58, 261; and J. P. Kenyon, *Robert Spencer, Earl of Sunderland, 1641–1702* (London, 1958), 136n.

What was persuasive about the final text of the *Declaration of Reasons*? What was promised that would “gain the people’s affections?” William was depicted as the “Dutch Deliverer,” whose aim was to rescue the Protestant religion and the Anglican church and to restore the laws and liberties of the English nation, all so grievously assaulted. Avoiding a direct attack on James himself, the manifesto blamed his “evil counsellors” for these assaults. The violations of these counselors were enumerated, with first priority given to the use of the dispensing power, that “strange and execrable maxim,” said (quite erroneously) to have been “invent[ed] and set on foot” by James’s advisers. The persecution of the Seven Bishops appeared prominently in the list of other grievances, which included the suspension of the Test Act, the *quo warranto* proceedings (by which James aimed to control municipal affairs), and the elevation of Papists to high posts in all areas of the government. Using an example certain to appeal to Englishmen, the *Declaration* asserted that “the dissimil effects of this subversion of the Established Religion, Laws and Liberties in England appear more evidently to us by what we see done in Ireland.”

As for himself, William could not sit idly by. The legitimate position of his dear wife Mary and of himself in the succession, the affection shown him and Mary by Englishmen over the years, and the fact that he had been invited by a “great many Lords, both Spiritual and Temporal, and by many Gentlemen, and other Subjects of all Ranks” compelled him to come. But he came not as a conqueror. His army was not directed against the English people. Rather, it was a force large enough only to protect William from the “violence” of James’s evil advisers.⁴³ It would be kept under strict control and returned to Holland as soon as possible. William promised, moreover, that he would “concur in everything” that a “free and lawful Parliament shall determine.” In neither the first nor the second *Declaration of Reasons* did William specify a future role for himself or James II in the government. But in the *Second Declaration* he disparaged the “malicious insinuations” of persons who suggested that he had any other design than to procure a lasting settlement on a sure foundation which would avoid the danger of the nation’s relapsing into misery again. Also in this second manifesto he dismissed the reforms belatedly instituted by James and declared—in words that were to become highly significant—that the only meaningful remedy was by a “Parliament in a Declaration of the Rights of the subject that have been invaded, and not by any pretended Acts of Grace.”

Finally, using the “dirty trick” recommended by his advisers, William cast doubts on the legitimacy of James’s son. “To crown all,” the *Declaration* read, “those evil consellers . . . published that the Queen hath brought forth a

⁴³ The point on the succession was reinforced by a print depicting William and Mary beneath whom is a text in Dutch listing the rulers of England and Scotland. The text shows that the House of Nassau dates from the year 682 A.D., and indicates that William and Mary are in a line of succession that goes back to William the Conqueror, in the case of England, and to David, in the case of Scotland. BL. A Volume of prints, 504. 1. 10 (18v–19). In keeping with the idea that he was not a conqueror, William issued an order on January 2, 1689 that any of his soldiers who suggested that he had conquered England be punished; see the *Orange Gazette*, January 7–10, 1688/9, #4. In fact, William brought about 15,000 men; see Baxter, *William III*, 237.

son.” But, the manifesto continued, the new Parliament should investigate the matter, for “not only we ourselves but all the good subjects of the Kingdom do vehemently suspect that the Pretended Prince of Wales was not borne by the Queen.” It was a daring and outrageous charge. “This is worse,” fumed the indignant English ambassador at The Hague, “than the public invasion, and more unpardonable.” It could not have been written, he declared, but by “incarnat Divells.”⁴⁴ As events proved, the promise that the new Parliament would examine the birth of the baby was disingenuous. In this and other respects the *Declaration* shaped past events and present policies to serve the stadholder’s interests.

The *Declaration of Reasons* was everywhere in the fall of 1688. The manifesto appeared in four languages: English, Dutch, German, and French. It was printed at Amsterdam, Edinburgh, The Hague, Hamburg, London, and Magdeburg. Copies printed at The Hague bore the official imprimatur of the prince: “Printed at The Hague by Arnold Leers by special order of His Highness.” Altogether, twenty-one editions in the four languages appeared in 1688, eight of them in English. Intended, clearly, for an international as well as an English audience, the *Declaration* was widely dispersed on the Continent. Copies were handed directly to all ambassadors and ministers at The Hague, except the English and French representatives.⁴⁵ Through copies in the Dutch language William justified his undertaking to his Dutch subjects on the same grounds he had employed in asking Their High Mightinesses—the members of the Estates-General of the seven provinces, the governing body of the Netherlands—for support. In like manner through the German version he informed the German people of his project in the same general terms he had used in soliciting help from the German princes. And the French translation of the manifesto appealed to Huguenots on the Continent as well as to those who had emigrated to England after the revocation of the Edict of Nantes in 1685.⁴⁶

The *Declaration* was distributed from one end of the British Isles to the other, from the southeastern downs to the northwestern highlands (for which a version aimed at the Scots without antagonizing the English was prepared by Burnet with William’s active involvement), and “many thousand copies” were sent across the Channel.⁴⁷ A pass word—“I come from Exeter”—was devised for the prince’s agents and their contacts in England to protect them from “speaking to any wrong person who brings the papers.” Specific instructions about the timing of distributing the manifesto were issued, emphasis

⁴⁴ BL, Add. MSS, 41,816, ff. 249–49v.

⁴⁵ BL, Add. MSS, 34,487, f. 35. Continental archives (Dutch repositories excepting) still need to be searched for material on the Revolution.

⁴⁶ A paper to be used by foreign ministers in justifying Holland’s support of the expedition largely reiterated the *Declaration of Reasons*; see BL, Add. MSS, 41,821, ff. 271–72. The role of the Huguenots in the Revolution would be worth studying. Articles scattered through the *Proceedings of the Huguenot Society of London* deal with Huguenots during the period from different points of view. Also see the useful article by Roy A. Sundstrom, “Some Original Sources Relating to Huguenot Refugees in England, 1680–1727,” *Albion*, 6 (1976): 3–9.

⁴⁷ Burnet, *History of His Own Time*, 3: 301, 302; and BL, Add. MSS, 41,816, ff. 209v, 237, 261.

being placed upon releasing the document after the prince had landed and not before.⁴⁸ Friends of the prince were given as many as three thousand copies and asked to distribute them in their counties and among their friends. Bundles of free copies were sent to booksellers who were invited to sell them at their own profit. Copies were posted through the penny post. Additional copies were turned out by at least one English printer, a John White of Yorkshire. The first, and for a time the only, English printer of the *Declaration*, White was richly rewarded by William after the prince became king with a monopoly in the city of York and the five northern counties for printing all notices concerning revenue and justice which the government might issue.⁴⁹

Other steps taken to put the tract before the public also reveal its importance to William and his friends. Burnet, for example, read the *Declaration* from the pulpit of Exeter Cathedral on the Sunday after the prince's entourage entered the city, and then sent it "in the Prince's name" to all the clergy "commanding them . . . to read it." Some people in Exeter circulated the *Declaration* and were arrested by the mayor for their trouble. All around England William's partisans read and posted the *Declaration*. In Cheshire, Henry Booth, Lord Delamere, "himself read . . . the *Declaration* at the Market Cross." At the garrison at Plymouth the *Declaration* was read to the officers and soldiers, who declared for the prince by "throwing up their hats and huzzas," and then it was posted on the gates of the citadel. It was also read in Falmouth where it met with "universal acclamations of joy."⁵⁰ In one way or another, then, England was saturated with the *Declaration*. As one contemporary wrote, it "passed into the hands of the generality of the nation."⁵¹

The first and second *Declaration of Reasons* were not the only pamphlets written under the direct supervision of William and his friends. *Letters*, in the form of printed broadsides, were addressed to the English army and fleet. These broadsides urged the men to join the prince in the defense of the nation's religion, laws, and liberties, promised them reward if they came in "seasonably," and counseled them not to allow a misplaced sense of loyalty to James to deter them. The letters were signed by William as "Your truly well wishing and affectionate Friend." Three tracts bearing the imprimatur "By Authority" were released. Written by Burnet, they dealt, in ways favorable to William, with specific questions that were troubling thoughtful Eng-

⁴⁸ Japikse, *Correspondentie van Willem III en van Hans Willem Bentinck*, 24: 618–19.

⁴⁹ BL, Add. MSS, 41,816, ff. 263–63v; John Carswell, *From Revolution to Revolution: England, 1688–1776* (London, 1973), 29; Singer, *Correspondence of . . . Clarendon*, 494; and *Calendar of State Papers, Domestic* (hereafter *CPSD*), 1689–90, 122. Also see C. H. Temperley, *A Dictionary of Printers and Printing* (London, 1839), 572.

⁵⁰ Dover, *Ellis Correspondence: Letters Written during the Years 1686, 1687, 1688*, 296, 308; the Carl H. Pforzheimer Library, Bulstrode MSS, vol. 12, Newsletters, Nov. 9, 1688, 2d letter; the *English Currant*, December 14–19, 1688, #3; and the Huntington Library, Hastings MSS, HA, 6074, 6075 (almost all of this material has been printed in HMC, *Report on the Manuscripts of the Late Reginald Rawdon Hastings*, 2: 197, 199). For other examples, see *CSPD*, *James II*, 3: #s 2005, 2044, and Morrice, "Entr'ing Book," 369. In addition, a print of William's landing at Torbay by the Dutchman Carolus Allard depicted the reading of the *Declaration of Reasons* to an assembled crowd, a fact which is not mentioned in any other source and may, therefore, be doubted.

⁵¹ *Quadriennium Jacobi: or, The History of the reign of James II from his first coming to the crown to his desertion* (London, 1689), 223. The tract was licensed and entered according to order.

lish people: the limits of obedience to civil authority, the right of resistance, the potential problems associated with recalling James, and the flight of the king.⁵² Further, Burnet undertook to answer a tract from James II's court which disparaged William's manifesto. His *A Review of the Reflections on the Prince of Orange's Declaration* was printed "By the Prince of Orange's special command."⁵³ And, although William repudiated it, a third *Declaration of Reasons*, dated November 28, 1688 at Sherburn Castle, was believed to have come from the prince's camp. This tract was a hysterical attack on the papists: it insinuated that they intended to set fire to London and Westminster and, with the help of French and Irish troops, massacre all the people there; and it called upon all good Protestants to seize, disarm, and imprison their Catholic neighbors. Some thought that this tract was partly responsible for inflaming the London mob in December.⁵⁴

William was really prepared to keep the presses rolling for his cause after he landed in England. What better proof is there of this intention and his interest in propaganda than the fact that he brought a printing press with him—along with soldiers and horses—as part of his invasion equipment? Within three weeks of his arrival, during the time he was at Exeter, it was reported that he was printing "two gazettes a week."⁵⁵ Still further, great quantities of tracts, among the hundreds of anonymous pieces that poured from the press, argued the case for William. Similarly, the eight new newspapers that appeared between December and March reported the news in ways biased toward the prince.⁵⁶ It has not been possible to discover precisely the number of tracts that were printed during these months, nor to say exactly how many copies of a particular pamphlet were printed. But contemporaries noted the profusion of pamphlets, enterprising publishers almost immediately reprinted the "most considerate" of them so that they might not "lie buried in a crowd of

⁵² An eighteenth-century historian wrote that the letter to the army was "spread underhand over the whole kingdom" and had a "wonderful effect" on the soldiers, in that it persuaded men who did not join Prince William when he landed not to fight for King James II until a free Parliament was called; John Banks, *The History of the Life and Reign of William III* (London, 1744), 207. The draft and the fair copy of the Letter to Seamen, signed by the prince on September 29, illustrate the care necessary to achieve correct English syntax and spelling; NUL, Portland MSS, PwA, 1663, 1664. The titles of the three tracts "By Authority" are (1) *An Enquiry into the Measures of Submission to the Supreme Authority and of the Grounds upon which it may be lawful or necessary for Subjects to defend their Religion, Lives, and Liberties*, (2) *An Enquiry into the Present State of Affairs: and in particular, Whether We Owe Allegiance to the King in these Circumstances? And whether we are bound to treat with him, and call him back, or Not?*, and (3) *Reflections on a Paper intituled His Majesty's Reasons for withdrawing himself from Rochester*. For attribution to Burnet, see H. C. Foxcroft and T. W. S. Clarke, *A Life of Gilbert Burnet, Bishop of Salisbury* (London, 1907), 244 and app. 2, 539.

⁵³ Foxcroft and Clarke, in their *Life of Gilbert Burnet*, app. 2, 538, note that this tract was printed in England after November 5, 1688.

⁵⁴ "Newdigate Newsletters," LC 1938; and Burnet, *History of His Own Time*, 3: 338–39 and n. The *London Courant*, December 12–15, 1688, #2, carried an emphatic denial of William's responsibility for the tract. The authorship has been attributed to Hugh Speke and the Rev. Samuel Johnson, the political pamphleteer. For the former, the *Dictionary of National Biography* contains useful data; the latter is not noticed. Speke, who "controlled" or, perhaps, owned a press, claimed authorship, but the claim has been discounted on the ground that it was entered as a means of currying favor.

⁵⁵ "Newdigate Newsletter," LC 1938.

⁵⁶ George Watson, ed., *The New Cambridge Bibliography of English Literature* (Cambridge, 1971), 2: 1318–19, lists the titles and dates of publication of these newspapers. In the United States, copies of almost every issue of all the papers, with the exception of the *Harlum Currant* (which lasted for only two issues), can be found at the Huntington Library.

pamphlets,” and contemporary collections testify to both interest and impact.⁵⁷ It seems beyond dispute that hundreds of printed pieces were in circulation and that a very large number of them favored William.⁵⁸

The pamphlets were, of course, addressed to a literate public. Lawrence Stone has estimated literacy for these years at about forty per cent of the adult males nationally and at about sixty-seven per cent of adult males in the cities. Potentially, then, these tracts reached not only the politically and socially elite members of English society, but also the nonelite—surely the middling ranks, and, perhaps, the illiterate masses as well.⁵⁹ Since many of them sold for a penny, they were within the reach of many people.⁶⁰ But precisely how many people bought them is not known. Nor is it known how many people the pamphlets actually reached through being read aloud in coffee houses which, with the breakdown of government controls, had resumed their role as scenes of political discussion and the dissemination of views, or through being handed on from hand to hand, as one broadside specifically instructed its readers to do.⁶¹ Various steps were taken to attract people of diverse levels of political sophistication. Apparently to simplify complex issues and win readers, authors crafted dialogues and devised inviting titles. For example, one anonymous pamphlet, *A Political Conference Between Aulicus, a Courtier, Demas, A Countryman, and Civicus, a Citizen: Clearing the Original of Civil Government, the Powers and the Duties of Sovereigns and Subjects, In a Familiar and Plain Way, which may be understood by every Ordinary Capacity*, clearly aimed through its title to appeal to readers of “every ordinary capacity.” Some humor was consciously used for political purposes, as in the *Roman Post Boy*, a kind of early comic book which freely indulged in bawdy jokes about Catholics, Jesuits, and Irish and

⁵⁷ *A Compleat Collection of Papers, In Twelve Parts: Relating to the Great Revolutions in England and Scotland, From the Time of the Seven Bishops Petitioning K. James II, against the Dispensing Power, June 8, 1688, to the Coronation of King William and Queen Mary, April 11, 1689* (London, 1689). “To the Reader”: the printer was Richard Janeway, and Richard Baldwin brought out a rival collection. There were several private collections: for example, the collection of Narcissus Luttrell (BL. C. 122. 1.5) and that of the then young barrister and member of the Convention Parliament, John Somers, later lord chancellor; see note 35 above. In the early eighteenth century, Humphrey Bartholomew (b. 1702), of University College, Oxford, collected 50,000 pamphlets, including many from 1688–89; the Bartholomew collection can be found at the Bodleian Library, Oxford. Also see NUL, Portland MSS, Pw.A. 2120, 2141, 2143 for figures of the number of copies of some tracts. For contemporary comments about their profusion, see Luttrell, *Brief Historical Relation*, 497; Anchitell Grey, *Debates of the House of Commons from the year 1667 to the year 1694*, 9 (London, 1763): 63; and E. Hickeringill, *A Speech Without-Doors: or, Some Modest Inquiries Humbly Proposed to the Right Honourable the Convention of Estates at Westminster, Jan. 22, 1688/9* (London, licensed Jan. 17, 1688/9), 32.

⁵⁸ Over a hundred tracts were reprinted in *A Complete Collection of Papers in Twelve Parts*. There are many other pamphlets and tracts in the British, the Bodleian, the Folger Shakespeare, and the Huntington Libraries. For a guide to the tracts in the Dutch archives, many of which were translated into Dutch and reprinted in Holland, see W. P. C. Knuttel, comp., *Catalogus van de Pamfletten-Versameling Berustende in de koninklijke bibliotheek* (’s-Gravenhage, 1895–1916), vol. 2, pt. 2 (1668–88), and vol. 3 (1689–1713).

⁵⁹ Stone, “Literacy and Education in England, 1640–1900,” *Past & Present*, #42 (1969), 109, 112, 125, 128. For comments about the diffusion of ideas from literate to illiterate or marginally literate groups, see R. S. Schofield, “The Measurement of Literacy in Pre-Industrial England,” *Literacy in Traditional Societies*, ed. Jack Goody (Cambridge, 1968), 312–13 and n. 1, and Davis, *Society and Culture in Early Modern France*, 72–73, 189, 226, 241.

⁶⁰ The price is noted on tracts which Narcissus Luttrell preserved; see note 57 above.

⁶¹ Bodl., Rawlinson MSS, D. 1079, f. 4v; no copy of this broadside seems to have survived. And see Peter Fraser, *The Intelligence of the Secretaries of State and Their Monopoly of Licensed News, 1660–1688* (Cambridge, 1956), 131–32.

served up malicious parodies of the Irish accent. To appeal to other tastes, printed accounts, all favoring William, of the activities of the Convention Parliament appeared, despite parliamentary orders forbidding them.⁶²

Efforts were made to promote wide circulation of the tracts. Some pamphlets advertised others. The *London Gazette* and some of the new newspapers also carried advertisements. Some tracts were hawked about the streets of London and dropped, according to personal testimony, at the feet of the customer.⁶³ Many of the tracts circulated during the elections to and the sitting of the Convention—at a time when interest in public affairs must have been keen. A few broadsides were addressed directly to the electorate, which numbered between 200,000 and 250,000 voters, while others were addressed directly to members of the Convention.⁶⁴ An unsuccessful attempt was made to distribute copies of a tract in the antechamber of the House of Commons. One pamphlet, according to its author, was handed directly to members of the Convention, while still another “was delivered” to the House of Commons just before an important vote. Just the presence of this printed matter ensured more discussion of the issues during the elections to the Convention Parliament than has been credited.⁶⁵ Parallels between the tracts and the debates in the Convention suggest, furthermore, that the pamphlets had an influence on what was said in Westminster. But, clearly, the printed material reached not only the decision-makers but people outside elite categories as well.

That so much material in William’s favor circulated can be explained in part by the freedom the press enjoyed during the months of revolution. Although the Licensing Act of 1662 was theoretically in force (since James II’s Parliament revived it in 1685), the various agencies and procedures for controlling printed matter had broken down, a condition which underscores the political upheaval experienced throughout the nation. Printers and publishers, some of whom had been involved in opposition politics in earlier years and who favored William’s cause, exploited this situation.⁶⁶ It has not been possi-

⁶² Other examples with inviting titles include *A Friendly Debate Between Dr. Kingsman, a Dissatisfied Clergyman and Gratianus Trimmer, a neighbor minister* (London, 1689); *A Plain and Familiar Discourse concerning Government. Wherein it is Debated, Whether Monarchy or a Commonwealth be best for the People* (n.d., n.p.); and *A Dialogue Between Dick and Tom. The Roman Post-Boy: or, Weekly Account from Rome* was printed by George Croom for John Mumford in March and April, 1689; in the United States all of these issues can be found at the Huntington Library. Also see Lois G. Schwoerer, “Press and Parliament in the Revolution of 1689,” *Historical Journal*, 20 (1977): 545–68.

⁶³ The *London Gazette*, February 4–7, 1688/9, #2425; the *London Intelligence*, January 19–22, 1688/9, #3; the *Orange Gazette*, February 5–8, 1688/9, #10; and *A Dialogue Between Dick and Tom*, 10.

⁶⁴ *Considerations Proposed to the Electors of the Ensuing Convention*. E. Bohum endorsed this broadside, “spread by a soldier who came over with the Prince of Orange who knew nothing of it,” and dated it January 9, 1688/9. A copy can be found in the Cambridge University Library, Selden MSS. 3, 235. Also see *A Seasonable Memento to all the Electors of Knights, Citizens, & Burgesses of England, for the approaching Convention to meet the 22 of this instant January 1689* (London, 1689). Cf. *A Brief Collection of some Memorandums: or, Things humbly offered to the consideration of the Great Convention and of the succeeding Parliament* (London, 1689), and *Proposals Humbly offered To the Lords and Commons in the present Convention, for Settling of the Government* (London, 1689).

⁶⁵ Morrice, “Entr’ing Book,” 449. The man was arrested; John Humfrey [?], *Advice Before It Be Too Late* (London, 1689), unpaginated. Bodl., Rawlinson MSS. D, 1079, f. 4v. See Plumb, “The Elections to the Convention Parliament of 1689,” 251. Cf. Horwitz, “Parliament and the Glorious Revolution,” 40–41, which offers an adjustment to Plumb’s analysis based on the number of contested elections—a total of sixty.

⁶⁶ Richard Baldwin and Richard Janeway provide two examples. Further research on the printers and publishers during the Revolution of 1688–89 is needed. Of them only Baldwin has been studied system-

ble to determine which, if any, of this material William or his close advisers commissioned. It is entirely likely that much of it was printed independently and therefore should not, strictly speaking, be regarded as "propaganda" because it was not part of an organized effort. But, whatever their origins, these tracts and newspapers had the effect of reinforcing the interpretation of William and his purposes which he and his close advisers had developed.

Although William's cause was well served by an uncensored press during this time of crisis, the prince attempted to control what was printed by trying to reinstitute the Stuart administrative procedures for censoring publications. Early in January he issued an order instructing the master of the Stationers Company and others to search out all "false, scandalous, and seditious books, papers of news, and pamphlets" and to bring the persons responsible for them to justice.⁶⁷ His effort had little apparent impact upon the flood of unlicensed tracts, but it does testify to his interest in shaping public opinion.

As a publicist, James II was no match for William III. James's position was seriously weakened because so little printed material promoting his side was available. James was not entirely blind to the power of public opinion; earlier in his reign he had rigorously enforced the already established laws and procedures for censoring printed matter. And, in the spring of 1688, the court inspired rumors, commissioned tracts, and dispersed *gratis* quantities of the king's "papers." As the crisis deepened in October George Jeffreys (James's lord chancellor) ordered coffee houses, upon pain of forfeiting their licenses, to keep no written news but the official *London Gazette*; and James issued a proclamation forbidding the discussion of political affairs by writing, printing, or speaking. In November, perceiving the strong attraction of William's declarations and letters, the king banned them absolutely, declaring it treason for anyone to read, receive, conceal, publish, disperse, repeat, or hand about any of the prince's printed pieces.⁶⁸ In addition, to counteract the first *Declaration of Reasons*, the Privy Council sat the afternoon of the day William landed, drawing up a rebuttal which was rushed to the press the next day. At least three answers to William's *Declaration* appeared: *Animadversions*, *Reflections*, and *Remarks*. Further demonstrating his respect for public opinion, James did an about-face with respect to the *Declaration*. In early December, when it became evident that the proclamation against Prince William's manifesto was ineffectual, James "to the astonishment of everyone," ordered that the *Declaration* be printed accompanied by *Remarks* and *Animadversions*. At the

atically; see Leona Rostenberg, "Richard and Anne Baldwin, Whig Patriot Publishers," *Papers of the Bibliographical Society of America*, 47 (New York, 1953): 1-42. Useful data are also in NUL, Portland MSS, 2159-2162, 2167.

⁶⁷ The order was printed in the *London Gazette*, January 7-10, 1688/9, #2417, and noticed by Morrice, "Entr'ing Book," 427.

⁶⁸ Robert Steele, ed., *Bibliography of Royal Proclamations of the Tudor and Stuart Sovereigns and of Others Published under Authority, 1485-1714*, (Oxford, 1910): #3888; issued October 26, 1688, this proclamation was described as a "padlock" on the news in *A Dialogue Between Dick and Tom*, 4. And Steele, *Bibliography of Royal Proclamations*, #3891; issued November 2, 1688, it was reprinted in the *London Gazette*, November 1-5, 1688, #2396. Dover, *Ellis Correspondence: Letters Written during the years 1686, 1687, 1688*, 272-73. See Frederick S. Siebert, *Freedom of the Press in England, 1476-1776: The Rise and Decline of Government Controls* (Urbana, Ill., 1952), pt. 4. For James's activities in the spring, see NUL, Portland MSS, PwA, 2141, 2147, 2159, 2161, 2167.

same time James exploited the *London Gazette*, the official government newspaper; it printed items "in the most hateful manner" to discredit William.⁶⁹ The vicious attack by the mob in mid-December on the house of the king's printer—during which the mob not only destroyed the house but also burned forms, letters, and upwards of 300 reams of paper, printed and unprinted—suggests that the king's tactics had some effect. But as James's government faltered it was unable to control the press, and the king's flight meant the entire collapse of restrictions. While partisan material for William flooded the presses, only a few tracts sympathetic to James appeared.⁷⁰ To the detriment of James's chances, there was no genuine debate in the pamphlets.

PAMPHLETS WERE NOT THE ONLY INSTRUMENT USED IN THE PROPAGANDA EFFORT. The themes announced in the tracts were reiterated and reinforced in printed pictures and commemorative medals. At least forty-nine prints, two sets of playing cards (a form of the print), and thirty-one medals were devised to support William's cause.⁷¹ With few exceptions, the designers were Dutch. This iconographic material is drenched in allegory and symbolism. Some of the symbols are complex and subtle, others quite straightforward. Some are readily recognizable as deeply indebted to the tradition of the Emblem Books which had enjoyed great popularity in the Renaissance. For example, the eye in the heavens with beams of light extending from it signified Providence in the prints, just as it had in the Emblem Books, while the decayed or cut down tree or, alternatively, the flourishing tree symbolized the condition of the commonweal.⁷²

The prints and medals were invariably accompanied by an explanatory text, so that if the viewer were puzzled, he had only to refer to the text. As in

⁶⁹ BL, Add. MSS, 34,510, 4: 265–67. *Reflections* was in print as early as November 13; *ibid.*, 246. It was powerful enough to warrant a reply from Burnet at William's special command; see page 836 above. For the effort of the Privy Council, see the Carl H. Pforzheimer Library, Bulstrode Newsletters, XII, November 5, 1688 (2d letter); and Dover, *Ellis Correspondence: Letters Written during the Years 1686, 1687, 1688*, 279.

⁷⁰ The *English Currant*, December 12–14, 1688. #2. For James's partisan tracts, see, for example, *The Debates in Deposing Kings; and of the Royal Succession of Great Britain* (London, 1688); *England's Crisis: or, The World well mended* (London, 1689); *An Honest Man's Wish for the Prince of Orange* (London, 1689); and [Dr. William Sherlock] *A Letter to a Member of the Convention* (London, 1689).

⁷¹ Two major modern catalogues which include the prints and medals connected with the Revolution are (1) *Catalogue of Prints and Drawings*, vol. 1, and (2) Edward Hawkins, comp., *Medallic Illustrations of the History of Great Britain and Ireland to the Death of George III*, ed. Augustus W. Franks and Herbert A. Grueber (London, 1969). An earlier catalogue is Gerard van Loon, *Histoire métallique des XVII provinces des Pays-Bas* . . . , 5 vols. (La Haye, 1732–37), transcribed from the Dutch, to which the British Museum catalogue is indebted. Although the material referred to in these catalogues is representative, a project to identify and catalogue all of the prints and medals associated with the Revolution would be worth undertaking. See, for example, BL, shelf number 504.1.10, a volume of unbound prints, and the collection of prints at the Guildhall Library in London and of medals at The Hague. For examples of the few medals aimed against Prince William, see *Catalogue of Prints and Drawings*, 745.

⁷² Other emblems which appeared in the prints and medals include the arm or the hand extending from the clouds and the wreath of laurel or oak. Emblem Books consisted of allegorical pictures, each accompanied by explanatory words, which together conveyed a moral, religious, or political idea. The first Emblem Book, the work of Andrea Alciati, appeared in Italy in 1531. Imported during the reign of Queen Elizabeth into England, this literary form flourished there until the end of the seventeenth century. For an introduction to the Emblem Books, see Rosemary Freeman, *English Emblem Books* (London, 1948). Also see George, *English Political Caricature to 1792*, 7–8.

the tracts, the language used on the prints was either English or Dutch, or English and Dutch, or sometimes English, Dutch, and French. Similarly, the medals were inscribed with words in Dutch, English, French, or Latin. The explanatory text made this material into what Dorothy George has called “graphic pamphlets.”⁷³ Many of the prints and medals are amusing. Their humor and exuberance must have delighted and, at the same time, instructed seventeenth-century viewers, even as clever political cartoons do today. As has long been recognized, nothing can be more politically damaging than ridicule. Furthermore, the presentation of ideas in these devices is much sharper and harsher than in most of the tracts. A picture on a print or medal can rarely qualify the point it is conveying.

No twentieth-century political figure has suffered more from the cartoonist's pen than did James II. The iconographic material depicts James as a captive of the Jesuits, a confederate of Louis XIV, and a king who tried to destroy the Protestant religion and the Anglican Church and to overthrow England's laws and government. A Dutch print, “Qualis vir Talis Oratio,” forcefully conveyed these points and, significantly, carried the date October 16, 1688 (fig. 1). The three columns of Dutch verse beneath a very busy and complicated picture (typical of Dutch prints) explain the meaning of the drawing. Although James is not, strictly speaking, caricatured⁷⁴ and there is no uglification of his features, he is made both ridiculous and malevolent. He is shown lying on a couch decorated with such figures as owls (signifying dark, nocturnal habits and underlying stupidity) and dragons (the symbol of Satan and evil power). On the king's head is a four-cornered hat (the Jesuit hat) encircled by the royal crown of England. James is vomiting horrid-looking reptiles, some of which are also wearing Jesuit hats. The designer has put words into their mouths. They cry out, “Jesuit Colleges,” “French Alliance,” “No Free Parliament.” A masked Jesuit—suggesting sinister, underhanded intentions—hands the Anglican bishops a sealed envelope. In the center of the picture, the lord mayor and aldermen of London hold their noses against the noxious odors of both Catholicism and James himself and turn away in disgust. Off in the left-hand corner is James's baby boy on the lap of a nurse with an amorous priest leaning over her. One rumor about the baby held that it had been fathered by a Jesuit, perhaps even by Father Edward Petre, James's Jesuit adviser and confessor. The nurse is feeding the baby from a bowl inscribed “Extermination of the Protestants.” The windmill with which the baby is playing alludes to another rumor—that the boy was really the son of a miller. The windmill appears repeatedly in prints and medals. In the upper right-hand corner in the distance with the sun rising sits William's fleet preparing to sail. The Dutch Deliverer to the rescue! The view of William is patent. The charges against James are unmistakable. Could any seventeenth-century viewer of this print fail to grasp its meaning?

⁷³ George, *English Political Caricature to 1792*, 3.

⁷⁴ George, in *English Political Caricature to 1792*, 62, writes that the word “caricature” is “now possibly permissible if incorrect”; also see pages 12–13. Because English artists were little involved, George devotes only two pages (62–64) to the prints associated with the Revolution of 1688–89.

These same themes appear in another print, *England's Memorial: Of its Wonderfull deliverance from French tyranny and Popish oppression, Performed through All mighty Gods infinite goodness and mercy. By His Highness William Henry of Nassau, the Iligh and Mighty Prince of Orange, 1688* (fig. 2). This is one of the few surviving prints designed by Englishmen.⁷⁵ In contrast to the Dutch prints, its composition is less crowded and the symbolism less exotic. In the center stands a flourishing orange tree—the symbol since 1641 of the House of Orange—obviously signifying the goodness and strength of William.⁷⁶ All around is evidence of the dangers England faced and the success of William in repulsing them. In the upper right-hand corner appears the Church of England, plainly awry, about to topple over. Above it is the eye of Providence, one ray of which bathes the orange tree and promises, “My blessing shall attend thee every where.” A voice from the church says, “Under this blessed shade [of the tree] I breath againe.” Opposite the eye of Providence in the upper left hand corner sits a council of devils and Jesuits. One remarks, “This cursed plant [the orange tree] has Sau’d the Heretick Church.” Another regrets, “But one blast more and the work was don.” Beneath them Louis XIV cuts off the heads of his subjects, plainly illustrating what English subjects might expect from a Catholic king! Louis advises James, “Tread on my Stepps and be great.” Near the tree stands James II, looking startled. An orange has knocked off his crown! He is fleeing preceded by his queen carrying the baby boy who holds a windmill. Mary says, “How the smell of this tree offends mee and the Child.” Closer to the trunk of the tree cluster some figures, among them a commoner pleading, “Heale our breaches.” Nearby the lord chancellor (who as judge had been responsible for very brutal sentences in the west following the invasion of the Duke of Monmouth in 1685) has been knocked down by an orange. On the right papists and Jesuits run from the hand of justice. They cry, “Hye for France,” “How strong it [the tree] Smells of a free Parliament,” “And that’s rank poison to a Jesuit’s nose.”

James’s baby son was as viciously maligned as James himself. No opportunity was lost to portray the baby as the offspring of a Jesuit priest or of a miller or sometimes as having been brought into his mother’s bed concealed in a warming pan. At least fourteen prints and medals appeared that perpetrated the libel that the boy was a fraud.⁷⁷ One medal, like some of the prints, implied that the baby was the son of both a Jesuit and a miller! On one side

⁷⁵ Another English print depicted the capture of Jeffreys: “The Lord Chancellor taken disguised in Wapping,” dated December 12, 1688. See *Catalogue of Prints and Drawings*, 723, which lists the five surviving copies of this print and indicates the differences among them. Jeffreys is shown with his arms pinioned by two men, surrounded by a crowd of people. Members of the crowd cry out, “Remember ye West,” “Knock his brains out.” There are other scurrilous details. The hatred of Jeffreys surfaced during the Revolution. William recounted that when he landed more than fifty women kissed his feet and begged him to put Jeffreys in their hands because he had hanged their husbands; van Terveen, “Verbaal . . . Witsen,” 136. Further, a petition concerning the chancellor was presented to the Convention; and it was printed as a broadside: *The Humble Petition of the Widdows and Fatherless Children in the West of England [for the punishment of Lord Chancellor Jeffreys]. Presented to this present Convention* (London, 1689).

⁷⁶ George, *English Political Caricature to 1792*, 44.

⁷⁷ See, for example, *Catalogue of Prints and Drawings*, 713. Although the warming pan theme is not particularly prominent in the visual materials, it did circulate as early as June 13; see NUL, Portland MSS, PwA, 2167.

sits the baby with a windmill on his head, held in the arms of Father Petre, who is himself seated on a lobster, symbolizing the order of the Society of Jesus (this from the tale that Ignatius Loyola accidentally dropped his Bible into the sea and it was recovered for him by a lobster). Near them is a French ship, signifying the connection of the Stuarts with the Sun King. The legend reads, "ALLON MON PRINCE NOUS SOMMES EN BON CHEMIN" (Let us go, my Prince, we are on the right road). The medal conveyed a false impression, for Father Petre had not taken the baby to France; the child had gone with his mother. On the other side of the coin is a heraldic shield. But, instead of showing the lion and the unicorn, the major symbols of the English monarchy, the shield bore a windmill. On the top of the windmill is no crown, but a Jesuit hat. A lobster serves as a badge, and a rosary—inscribed, "HONY. SOIT. QVI. BON. Y. PENSE." (Shame to him who thinks well of this)—is draped over all. The legend reads, "LES ARMES ET L'ORDRE DU PRETENDU PRINCE DE GALLES" (The arms and order of the pretended Prince of Wales).

Another theme reiterated in the iconographic material was that William had come to restore England's laws, liberties, and religion. One medal (fig. 3), for example, shows on one side an orange tree sheltering the figure of a woman, which since the Restoration medals of the 1660s had signified Britannia.⁷⁸ She is leaning upon a Bible to show that England rests upon the Protestant religion, and on her lance is a cap of liberty. A well-established device, the cap of liberty is of special interest. It had long been a Dutch symbol signifying their fight for liberty from their Spanish overlords in the sixteenth century. But its origins were much more ancient than that. The cap or bonnet was worn by all Roman citizens. At the time of the Roman Republic it was the symbol of the free man. Slaves put it on the moment they were freed.⁷⁹ The implications for England's plight are plain, but the words inscribed on a scroll near Britannia make the point explicitly: "BRITAN: ON-TROERT. WET. GEVELT. PRINC D'ORANGIE ONS HERSTELT." (Britain is troubled by the violated law; the Prince of Orange restores the law to us).

The reverse of this medal is equally explicit. On it appears the Belgic Lion, the symbol of Holland and the term applied to William in at least one tract and a commemorative poem. Holding in one paw seven arrows representing the seven United Provinces of the Netherlands, in his other paw the lion brandishes a sword entwined with the branches of an orange tree. One foot

⁷⁸ George, *English Political Caricature to 1792*, 9, 44–45. She states that the figure of Britannia, although of ancient lineage (deriving from Roman coins), was not used in graphic satire until after its appearance on Restoration medals. It may be noted that Britannia seated on her rock appears on the title page of the 1600 and 1607 editions of William Camden's *Britannia*, not first on the 1609 edition, as in George (page 9). Frances Stuart, Duchess of Richmond and Lennox was the model for the Restoration medals; see Cyril Hughes Hartmann, *La Belle Stuart: Memoirs of Court and Society in the Times of Frances Teresa Stuart, Duchess of Richmond and Lennox* (London, 1924), 142–44.

⁷⁹ George, *English Political Caricature to 1792*, 44; and C. Daremberg, E. Saglio, and E. Potter, comps., *Dictionnaire des antiquités grecques et romaines d'après les textes et les monuments* (Paris, 1905), 4, pt. 1: 479–81. This cap of liberty appeared on many other medals. For example, on the medal cast to commemorate the ceremony on February 13, William hands the cap of liberty to three female figures, who symbolize England, Scotland, and Ireland. The liberty cap reappeared as a symbol of the revolutionaries in both the American and French Revolutions.

rests on the Bible and the other crushes a serpent which seems to be attacking the Bible. On the ground is a column marked "MAG. CART." (Magna Carta), which has toppled over to signify that the laws of England are prostrate. In the distance is William's fleet approaching land where a church and houses stand. Once again, the message is clear: the Prince of Orange will restore England's religion and laws, both of which have been undermined.



Figure 3

In the course of maligning James and eulogizing William, the iconographic material supplied a visual record of episodes in the history of the Revolution. A print of the prince's reception in London, designed by Carolus Allard and issued "with privilege from the Great and Mighty States of Holland West-friseland," provides a striking example. Like some early Renaissance paintings, the print tells a story. In the center is a picture of William's reception in London, showing him being welcomed with great pageantry. This scene is a gloss on the truth. Contemporary observers did report that William was received with enthusiasm, but a devoted partisan of William's lamented the prince's failure to make a magnificent entry; and a friend of James commented that, upon the king's return to the city from his first flight, James had been received with much greater expressions of affection than William.⁸⁰ Around the central picture are little cartoons depicting episodes that led up to the reception, including the birth of the "pretended" baby. Beneath the central picture is the order of William's fleet as it sailed to Torbay, with a list of the names of those Englishmen who attended the prince. Thus, a seventeenth-century viewer had before him in one print a visual record of major incidents of the preceding few months. And once again, that record was slanted in William's favor.

Polemical playing cards also very effectively presented incidents in the Revolution. The use of playing cards for political purposes was rather new; such cards first appeared only in 1659. That pack satirized the Rump Parlia-

⁸⁰ "The Newdigate Newsletters," LC 1950; the *London Courant*, December 18-22, 1688, #4; the *English Courant*, December 14-19, 1688, #3; Morrice, "Entr'ing Book," 404; Burnet, *History of His Own Time*, 3: 358; and remarks as quoted in Robert Beddard, "The Loyalist Opposition in the Interregnum: A Letter of Dr. Francis Turner, Bishop of Ely, on the Revolution of 1688," *BHHR*, 40 (1967): 107. Romeyn de Hooghe also designed a print of the prince's reception in London. An appended text in both French and Dutch describes this elaborate picture. Guildhall Library, Print Room, Pageants L 22.2.

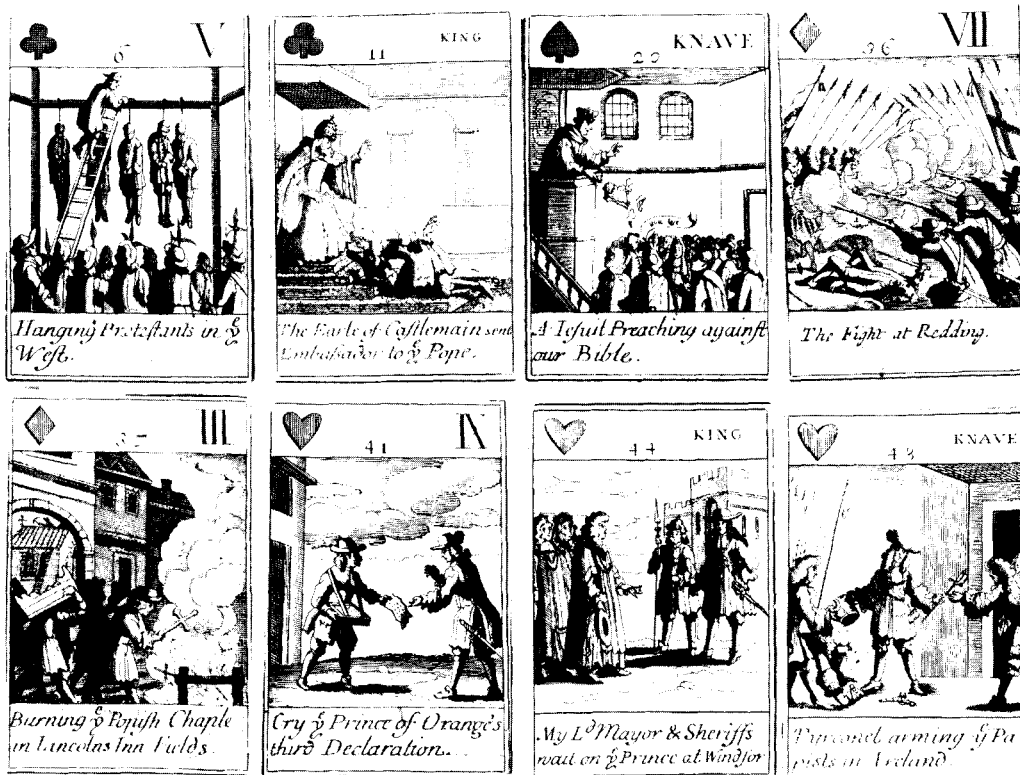


Figure 4

ment and was probably designed for the royalist colony in Holland. Cards also depicted the events of the Popish Plot, the Rye House Plot, and Monmouth's Rebellion. Two sets, both now incomplete, were printed early in 1689. One pack, advertised for twelve pennies in a February issue of the *Orange Gazette*, was said to represent in "lively cuts" the history of the late times.⁸¹ When the cards are arranged in an appropriate sequence (and numbers on one set indicate that sequence), they provide a pictorial narrative record of events. The cards reproduced here (fig. 4) reveal how lively were the cuts. With their simple figures, short text, and in some cases atrocious spelling, the cards could appeal to a very broad spectrum of people. One does not have to be politically sophisticated or well-to-do to understand the story the cards tell. Apparently brisk sales were expected, for five booksellers were specifically identified as carrying the cards. One pack—perhaps a rival at a cheaper price?—has less carefully executed pictures than the other. Francis Barlow, a book illustrator credited with the designs of the playing cards for the Popish Plot, designed certainly one, and perhaps both, packs for the Revolution. In

⁸¹ In one set, fifty out of fifty-two cards, and, in the other set, twenty-five out of fifty-two cards survive. Measuring three and one half inches by two inches, the cards are about the size of present day playing cards. The *Orange Gazette*, February 22–26, 1688/9, #15. For the earlier packs, see George, *English Political Caricature to 1792*, 52, 60, 62, 64.

his fascinating book *The Early Comic Strip*, David Kunzle comments upon these playing cards, seeing them as forerunners of the comic strip.⁸²

The potential audience of the prints and medals was very broad. In a society whose literacy rate was low, the iconographic material must have had a very important role to play in mass communication. The prints were capable of reaching a politically unsophisticated, even illiterate, element in society. Mass-produced and cheap, selling probably for three pennies,⁸³ many prints were surely posted on the doors of coffee houses or passed around from hand to hand. Moreover, as Dorothy George has stressed, not only do prints seek to influence public opinion, they also reflect that opinion. Since the most effective propaganda takes into account themes that may be expected to elicit a positive response, not only the pamphlets but also the iconographic materials offer a way of discovering the views of the nonelite, even the illiterate, in society.⁸⁴ At the same time the prints and medals were aimed at a politically conscious public from the middling ranks on up in both Europe (as the several languages suggest) and England. That advertisements for prints and medals appeared in the newspapers indicates a literate, middling to well-to-do English audience. The medals, cast variously in gold, silver, copper, lead, or bronze, were accessible to the middling ranks, but would have been more costly than the prints, starting at around ten shillings and going on up in price. But there was a potential market for medals, for some contemporaries had already formed sizable collections of ancient and modern commemorative medals.⁸⁵

Whether William or his close friends commissioned prints and medals is not absolutely certain, but circumstantial evidence suggests that they did. That most of the visual material about events occurring in England was done by Dutch designers is significant.⁸⁶ That the prints and medals provide a running

⁸² Kunzle, *The Early Comic Strip: Narrative Strips and Picture Stories in the European Broadsheet from c. 1450 to 1825* (Berkeley, Calif., 1973), 137, 144, 145.

⁸³ The price of the 1688–89 prints is unknown, but see the price of similar prints in Leona Rostenberg, *English Publishers in the Graphic Arts, 1599–1700: A Study of Print Sellers & Publishers of Engravings, Art and Architectural Manuals, Maps and Copy-Books* (New York, 1963), 45, 46, 48, 53, 74, 92.

⁸⁴ George, *English Political Caricature to 1792*, 1. The point is made by Myriam Yardeni, *La conscience nationale en France pendant les guerres de religion, 1559–98* (Louvain, 1971), 9–10.

⁸⁵ The *London Gazette*, December 31–January 3, 1688/9, #2415; the *Universal Intelligence*, January 1–3, 1688/9, #8; and the *Orange Gazette*, January 17–21, 1688/9, #6, all carried advertisements for the medals. Like the prints, the price of the 1688–89 medals is unknown, but a list of twenty-five other medals designed by John Roettier gives the price of each, which ranged from 10s. to £4 10s. Of the twenty-five, eight medals cost under £1. See "A List of Monsieur Roettier's Medals with Cases" (n.d.). Bodl., Rawlinson MSS, A, 179, f. 63. The gold coronation medal designed by John Roettier was worth £5 40s.; Evelyn, *Diary*, 633. And, as with the pamphlet literature, there is evidence of contemporary collections: see, for example, the letter from John Evelyn to Samuel Pepys, advising him on collecting medals and art; Henry B. Wheatley, ed., *Diary of John Evelyn to which are added a Selection from His Familiar Letters*, 3 (London, 1906): 435–56. Evelyn remarked that Elias Ashmole had collected "all" the ancient and modern English commemorative medals. Archbishop William Sancroft himself sketched the design for a medal to commemorate the acquittal of the Seven Bishops; Bodl., Tanner MSS, 28, f. 142.

⁸⁶ More work is needed on the Dutch designers including, in addition to the ones mentioned in this article, Jan Boskam, Jan Smeltzing, and John Roettier. Their backgrounds, their relationship with each other and with political leaders (William and others), their motives for engaging in polemical conflicts, and the degrees of success of their careers are among the questions that need to be asked. Some information may be found in Leonard Forrer, comp., *Biographical Dictionary of Medallists* (London, 1904; reprint ed., 1970). Also see C. van de Haar, "Romeyn de Hooghe en de Pamflettenstrijd van Jaren 1689 en 1690," *Tijdschrift*

pictorial account of events soon after they had occurred, invariably in ways favorable to William, invites the hypothesis that the designers were getting their information from sources close to the prince's court. That such prints and medals supplied visual representations for themes set out in the tracts written under the supervision of William and his close advisers, reinforces the hypothesis. That some of the prints bore the imprimatur of the Estates-General indicates an intimate connection between the designers and the Dutch government. And that William had previously used medals to commemorate every important event in his personal life up to 1688 adds strength to the idea that he encouraged the appearance of medals in 1688–89. After becoming king, William rewarded Romeyn de Hooghe, perhaps the most renowned of the designers, for a satirical print he had produced during the Revolution. William continued to employ Dutch designers and to use iconographic material to sway public opinion. De Hooghe designed at least forty-four prints for the new king and R. Arondeaux—the most talented, perhaps, of the medallists—was “much employed.” Moreover, King William appointed not only a royal engraver, a usual step, but also a royal medallist, the first time such an appointment had been made. During his thirteen-year reign, 221 prints and medals were produced, many of them Dutch and almost all favorable to William, a fact which surely testifies to his appreciation of their value. By comparison 182 prints and medals appeared during Charles II's twenty-five-year reign. There is no evidence that James II used iconographic material to sway public opinion. For his coronation only four medals, apparently, were issued, whereas the coronation of William and Mary was commemorated by at least twenty-eight. Only in 1685 did a handful of prints and medals appear that might be said to support James II. The rest of the sixty items that were in circulation during his four-year rule were either politically neutral or from William's camp.⁸⁷

DID ALL THESE TRACTS, PRINTS, AND MEDALS HAVE AN IMPACT UPON THE REVOLUTION of 1689? Did this polemical effort make any difference at all? The attempt to cosmetize William's appearance and personality apparently failed to convince the increasing numbers of people who came into contact with the prince. A woman who saw William for the first time did not find him handsome. She wrote that he was a “man of no presence” and “very homely at first sight,” but she admitted that if “one looks on him, he has something in his face both wise and good.” And at the coronation in April, it was commonly said that there had never been a more ugly king or a more beautiful queen.⁸⁸ Nor did

voor Geschiedenis, 64 (1956): 155–77. The rewards of such a study are suggested by a book on a similar topic: Rostenberg, *English Publishers in the Graphic Arts, 1599–1700*.

⁸⁷ Chevalier, *Histoire de Guillaume III, par médailles* (1692), demonstrates William's prior interest; also see *Catalogue of Prints and Drawings*, 716. The figures are based upon the entries in volume 2 of the *Catalogue*. Hawkins, *Medallie Illustrations*, 2: 720; and *CSPD, 1689–90*, 228, and *1690–91*, 185.

⁸⁸ The woman is quoted in Robb, *William of Orange*, 280. Another contemporary, said to be an admirer of both Charles II and James II, commented that William “hath more Ma[jes]tie in his appearance than both

William come across as affable, sweet-tempered, and charming. He could be ingratiating when it served his interests, but, in general, he remained taciturn and withdrawn. Some Englishmen, feeling that the prince was not forthcoming enough, asked a member of his Dutch entourage to “admonish” him. But the admonishment, if it were indeed delivered, did no apparent good. The diarist John Evelyn reported three different times how serious and reserved William was and commented at the end of January that English nobles and others were disappointed that they were not more graciously and cheerfully received. William, apparently, rebuffed familiarities. Reportedly, an English lord—a firm partisan of William’s—enthusiastically punctuated his conversation with the prince with several great oaths. William responded to the lord’s story by telling him he should not swear!⁸⁹ Things did not improve. Somewhat later it was said that the king was “shut up all the day long” while people waited impatiently to see him but that, when they were admitted to an audience, William’s “silence . . . distasted them as much as if they had been denied” an audience.⁹⁰

On the other hand, great success attended the attempt to depict the prince as the selfless Dutch Deliverer whose only purpose in coming to England was to call a free and lawful Parliament so that the religion, laws, and liberties of the nation might be restored; and concomitant success attended the effort to paint James II as a tyrant, bound to Jesuits and Louis XIV, and bent on destroying England’s religion, liberties, and law, even to the point of foisting a supposititious baby on the nation. These themes, set out most explicitly in the *Declaration of Reasons* and iterated and reiterated in many other tracts and in prints and medals, provoked a significant response from all sides. The appearance of the prince’s manifesto caused a frantic reaction from James II. Not only did he escalate his policy of controlling the press, he also displayed personal behavior verging on hysteria. James banned the *Declaration* absolutely and also burned all but one of the copies he had received. Thus he himself so nearly fulfilled the terms of his ban that it was extremely difficult for anyone about the court to get his hands on a copy. The king held several meetings with some bishops and peers to solicit public, written denials from them that they had, as the *Declaration* claimed, invited William to England. At these meetings, selected portions of the manifesto were read, but the king refused to allow anyone present actually to peruse his copy. But one day, he took Clarendon into his “closet” and let him look at the prince’s manifesto. Finally, Clarendon persuaded the Princess Anne to lend him her copy. She agreed on the condition that he would return it promptly, for Anne had induced James to lend her his only copy and she had to return it to him the next day! James testified further to the impact of propaganda on him: he attributed the defections in his army and the nation to the tracts, prints, and

the late Kings”; Morrice, “Entr’ing Book,” 428. For the coronation, see Krämer, “Mémoires de Monsieur B.,” 82; and “Journaal van Constantyn Huygens,” 112.

⁸⁹ Morrice, “Entr’ing Book,” 364, 438; “Newdigate Newsletters,” LC 1951; Luttrell, *Brief Historical Relation*, 493; van Terveen, “Verbaal . . . Witsen,” 135; and Evelyn, *Diary*, 612, 620, 625. The lord was John Lovelace, 3d Baron Lovelace of Hurley (1638?–1693), who was known to be impetuous.

⁹⁰ As quoted in Miller, *Life and Times of William and Mary*, 120.

medals—"all artes"—which had made him "appear as black as Hell"; and James justified his second flight by the claim that his safety was endangered by the man who had taken "such pains."⁹¹

Although William accepted the *Declaration of Reasons* with initial reluctance, the manifesto became a kind of position paper for his cause, and both the prince and his friends used it as a basis for discussing solutions to the crisis. That he would fulfill the terms of his manifesto William and his closest advisers, Bentinck and Willem Dykvelt, repeatedly affirmed at every crucial point in the course of the Revolution: when they received emissaries from King James II early in December, in important private conversations with such men as the Earl of Clarendon, when William addressed meetings of the Lords held in mid-December, when the Prince opened a meeting of lords and all members of Parliament who had served in the Parliaments of Charles II in late December, and in the letter which he sent to be read at the opening ceremonies of the Convention Parliament. And, in response, William's supporters repeatedly declared at these meetings, as Richard Hampden did on December 26 for example, that their actions were aimed at "attaining the end of your Highness's *Declaration*."⁹²

Some Tories, however, who hoped to achieve a solution to the crisis quite different from the one finally agreed to, initially dealt with William on the basis of his manifesto and were bitterly disenchanted when they later perceived that, in their view at least, the prince was not living up to its terms. The Earl of Clarendon wrote, "The Prince's *Declaration* gained him the hearts of the whole kingdom, but the conscience of many of the best men would not permit them to come into the measures taken since." Sir Edward Seymour, the great west-country Tory leader, said early in January that "all the West went into the Prince of Orange upon his *Declaration*, thinking in a free Parliament to redress all that was amiss. . . ." But, he continued, people now feared that the prince "aims at something else." Or again, a pamphleteer, styling himself the "State Prodigal" and unfriendly to William, bitterly criticized the prince for the "black methods" used in perpetuating publicly the rumors that the baby was fraudulent and that James was allied with France and for failing to investigate these charges, as promised. He charged William and his friends with "inventing . . . Stories, together with disguising and concealing Truth," and complained that they "laugh at us that we did not understand them sooner."⁹³ Disillusionment with the prince on the part of some Englishmen is directly traceable to the points—set out in the *Declaration* and reiterated in the visual material—that seemed not to have been fulfilled.

In other ways as well the *Declaration* had an impact upon the solution to the

⁹¹ Singer, *Correspondence of . . . Clarendon*, 199, 200, 494, 503; and J. S. Clarke, *The Life of James the Second, King of England, etc., Collected out of Memoirs Writ of his own Hand, Published from the Original Stuart Manuscripts in Carlton-House*, 2 (London, 1816): 274.

⁹² Singer, *Correspondence of . . . Clarendon*, 214, 215; H. C. Foxcroft, *Life and Letters of the First Marquis of Halifax*, 2 (London, 1898): 24; Morrice "Entr'ing Book," 384, 393; "Newdigate Newsletters," LC 1953; and *Journals of the House of Commons*, 10: 7, 9.

⁹³ F. J. Routledge, ed., *Calendar of the Clarendon State Papers in the Bodleian Library*, 5 (Oxford, 1970): 687; Singer, *Correspondence of . . . Clarendon*, 214, 218, 233-34, 238, 244, 246, 252; and *The State-Prodigal his Return: Containing a true State of the Nation. In a Letter to a Friend* (London, 1689), 1, 2.

crisis England confronted. To some extent, the manifesto limited William's political options, so that in a certain sense William really was hoisted on the petard of his own propaganda. For example, in December it was reported that "the lawyers" were "generally of the opinion" that the prince ought to declare himself king, as Henry VII had done. Then he could legally call a Parliament because he would be, in fact, king. But, as Burnet explained, William could not do that because it was contrary to his *Declaration* which referred the solution to the crisis to a Parliament. Burnet went on to say that, if William declared himself king, that "step would make all that the prince had hitherto done pass for aspiring ambition," would "disgust those who had hitherto been the best affected to his designs" and would make others who were "less concerned in the quarrel" assume he was a conquerer.⁹⁴ In other words, William really had no alternative—because of what he had promised in his *Declaration*—but to go forward with elections to a Convention Parliament. Such a course was politically astute, but as the prince had said in August, it was not without hazard. It cost William something. He wrote impatiently to a Dutch confidante during these weeks that, if he "were not by nature so scrupulous," he would have been "able to finish the affair soon," and, he added, "I have more trouble than you can imagine." The strain he was under manifested itself in a deepening of his asthmatic cough and a weight loss, both so marked that Mary was seriously concerned when she joined him.⁹⁵

William's propaganda also restricted his response to the Declaration of Rights, that lengthy statement of the nation's grievances and rights which was read to William and Mary at the ceremony on February 13, just before the crown of England was offered to them. The insistence in the prince's propaganda that he came only to redress grievances and restore rights and the specific statement in the *Second Declaration* that the "only means for obtaining a full redress and remedy of those grievances" was by a "Parliament in a Declaration of Rights of the subject that have been invaded" made it extremely awkward for William to resist the Convention's determination to present a statement about grievances and rights. In the Convention debates, members made the situation even more delicate by justifying their activities as fulfilling the prince's manifesto. A sentence in the Declaration of Rights avers that members were "particularly encouraged" by the prince's declaration to draw up their own claim of rights, because, as his manifesto had said, "the only means for obtaining a full redress and remedy" of grievances was by a "Parliament in a Declaration of the Rights of the subject that have been invaded." The prince's disapprobation, however, was keen. Along with other considerations, it accounts for the degree to which the final text of the Declaration of Rights was a much watered-down version of the first draft. And his anger was partly responsible for a sharp, if short-lived, crisis on February 8 and 9 which could have had the effect of changing and delaying

⁹⁴ Burnet, *History of His Own Time*, 3: 361-62.

⁹⁵ F. L. Müller, ed., *Willem von Oranien und Georg Friederich von Waldeck* (The Hague, 1873-80), 2: 122; and R. Doebner, ed., *Memoirs of Mary, Queen of England (1689-93). Together with Her Letters* (Leipzig, 1886), 11.

the solution to the national dilemma.⁹⁶ The depth of William's disapprobation was also reflected later, when, in discussing the bill that would transform the Declaration into statutory form and make it the famous Bill of Rights, he confessed that "he had no mind to confirm [all the articles in the Declaration], but the condition of his affairs overruled his inclinations."⁹⁷ William accepted the Declaration of Rights in February because, given the manifesto he had reluctantly approved, it would have been extremely awkward to do otherwise. His reluctance to restrict—either substantively or symbolically—the powers of the monarchy was sacrificed to achieve a public image compatible with his propaganda.

Finally, it is not too much to say that the propaganda effort of William and his friends (along with other considerations, of course) won for the prince the crown of England. Writing shortly after the event, a staunch friend of William's, Henry Booth, Lord Delamere, asserted that the crown was offered to William "not so much because he was the chief instrument of our deliverance, although we owe him much for that, but rather in hopes of having the effect of his *Declaration*." Delamere explained that it was in William's interests to do what he had promised in the manifesto and that the nation, therefore, was "more likely" to get grievances redressed by the prince than by any other person. William, Delamere thought, "could not but be sensible of the reproach and hazard he ran . . . [if] having fault with King James's administration . . . , he did not amend whatever was amiss."⁹⁸

ALL DURING THE MONTHS OF REVOLUTION WILLIAM WAS PORTRAYED AS A DELIVERER and James as a tyrant. An intensive propaganda campaign using every means at hand assured that most of what Englishmen were reading and seeing advanced this single point of view. That tracts, prints, and medals all repeated the same ideas reinforced their impact. These written and visual materials instructed, entertained, and enticed readers and viewers. By portraying William and his policies in the most favorable light and undergirding every step taken, they vindicated actions which, from another point of view, were treason. By asserting that the prince came only to assure the calling of a free Parliament which would settle the national crisis, these devices conveyed the impression that Englishmen held the fate of the nation in their own hands. As a result, in part, of this effort a broad consensus of approval for the Dutch prince and his policies, however short-lived that approbation proved to be, was achieved when it was most needed—when the crisis was being resolved. The campaign to shape public opinion, then, helps to explain William's success, which, whatever the view of Whig historians, was neither predestined nor inevitable. The campaign also had an effect, equally important in understanding the Revolution, on the nature of the settlement. Not only did the

⁹⁶ See Frankle, "The Formulation of the Declaration of Rights," and Horwitz, "Parliament and the Glorious Revolution," 48–49; Horwitz first revealed the crisis of mid-February.

⁹⁷ Foxcroft, *Life and Letters of . . . Halifax*, 217 (The Spencer House Journals).

⁹⁸ *The Works of Henry Booth, Lord Delamere, 1st Earl of Warrington* [1672–94] (London, 1694), 368–69.

propaganda restrict William's political options, it influenced also the terms of the Declaration of Rights and thus the Bill of Rights, the statutory form of the Declaration. The prince's effort, moreover, surely helped to avoid bloodshed in England during these critical months and thus contributed to the most distinguishing attribute of the Revolution, its nonviolent character. The success of the propaganda helps to explain how it happened that a solution to which, J. P. Kenyon has claimed, less than five percent of the governing class would have agreed if they had realized clearly what they were doing at the time was in fact accepted.⁹⁹

That tracts, pictures, and medals were the principal devices in the campaign is significant for several reasons. Relatively new instruments in the development of political propaganda, they underscore the importance of the great technological invention of the early modern era—the printing press. Although printed tracts and iconographic material had been employed earlier, not before 1688–89 had they been combined in such numbers toward a single political purpose. William and his partisans exploited a unique situation. Because of the prince's Dutch connection, he dominated the graphic arts and the medals. Because of his connection with English and Dutch publicists and printers, he also dominated the press. This campaign was the first example of the close association between England and Holland that would characterize the reign of William III, and William himself was the first king in England to use both printed and iconographic propaganda in an intensive and consistent way. Later during the eighteenth century men in and out of government continued the practice. The heirs of a long line of development, printed tracts appeared at every political controversy, and the cartoons of Hogarth and others played a prominent part in English public life.

William and his adherents aimed their propaganda at, and were successful in reaching, a broad spectrum of society—including literate, marginally literate, and, perhaps, even illiterate Englishmen far outside socially and politically elite categories. Such an audience underscores the political significance of the growth of literacy. The implementation of such a comprehensive campaign to shape public opinion also testifies to a recognition of the potential power of the lower classes. Those scholars who regard the Revolution of 1688–89 as a *coup d'état*, carried out by a very small number of people, have not understood the nature of the campaign William mounted. Finally, William's propaganda conveyed an interpretation of persons, motives, policies, and events that has, by and large, been accepted and perpetuated. There is no more indisputable testimony to its effectiveness than that the interpretation it advanced has dominated the scholarship of the Revolution ever since. One reason, then, that the Revolution of 1688–89 has for three hundred years been perceived as “glorious” is that William and his friends successfully contrived—through propaganda dispersed largely by pamphlets, prints, and medals—to make it appear so.

⁹⁹ Kenyon, *The Stuart Constitution, 1603–1689: Documents and Commentary* (Cambridge, 1966), 1.

The Junior Faculty in "Revolt": Reform Plans for Berlin University in 1848

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THE RECENT SURPLUS OF QUALIFIED YOUNGER SCHOLARS HAS CAUSED CONCERN that universities will become divided into an aristocracy of the tenured and a proletariat of the untenured, that academic departments or disciplines may stagnate while promising young talent goes to waste, migrating from one short-term appointment to another. By contrast, in nineteenth-century German universities the excess number of young academics was considered to be a sign of vitality; a burgeoning junior faculty was the nursery (*Pflanzschule*) of academic talent from which the most promising would be selected to fill comparatively few well-paid and prestigious professorships. This surplus of German academics, moreover, was not unique in Western Europe during the first half of the nineteenth century. "Too many men were educated for a small number of important and prestigious jobs." As the most industrialized nation, Britain could provide many careers in business as well as in colonial service. But such safety valves did not exist in Germany and France. There state employment conferred the status and security that made investment in an academic degree worthwhile. Many of those whose hopes for social and economic advancement by education were thwarted became part of an "intellectual proletariat" and a factor in the political instability of 1830 and 1848.¹

Although men who cannot market their education or professional skill may support radical changes during a revolution, they are more likely in normal times to be a force for conformism and stability. In Prussia the government could choose at leisure from a large number of candidates when appointing teachers, clergymen, doctors, jurists, or professors to tenured and salaried civil service positions. University graduates who sought state office faced long, usually unpaid apprenticeships during which senior officials regularly wrote confidential reports on the apprentices' performance and made recommendations for their promotion or dismissal.² The surplus of candidates assured that

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¹ Lenore O'Boyle, "The Problem of an Excess of Educated Men in Western Europe, 1800-1850," *Journal of Modern History*, 42 (1970): 477-78, 494; John R. Gillis, *The Prussian Bureaucracy in Crisis, 1840-1860: Origins of an Administrative Ethos* (Stanford, Calif., 1971), 14-15; and Reinhart Koselleck, *Preussen zwischen Reform und Revolution: Allgemeines Landrecht, Verwaltung, und soziale Bewegung von 1791 bis 1848* (Stuttgart, 1967), 439-40.

² Gillis, *Prussian Bureaucracy in Crisis, 1840-1860*, 24-27.

junior officials would strive to conform to the ethos of their superiors. In Prussia this outward conformity dissipated after the revolution in March 1848, when many younger civil servants stood for public office as liberals or democrats. But these junior bureaucrats were too dispersed to organize as a lobby for reforming their professions. The junior faculty of the universities, however, contained a “critical mass” of hopeful or thwarted candidates who could organize quickly to publicize the humiliation and injustice of their position and draft reform plans.

Of all German universities in 1848, Berlin witnessed the most severe confrontation between junior and senior faculty. Max Lenz, the university’s official historian, portrayed the conflict as an impertinent challenge to legitimate authority—an interpretation that reflected his commitment to the Prussian-German authoritarian tradition. Had Lenz seen the records of the junior faculty’s reform committee, he would have been even more disparaging.³ These papers give a new and more complete picture of the grievances and aspirations of Berlin’s junior faculty and allow a rare glimpse of some neglected and less admirable, though by no means exceptional, features of the much-admired nineteenth-century German university system. Moreover, the reform efforts of young academics like Rudolf Gneist and Rudolf Virchow reveal a good deal about the hardships of academic apprenticeship and help to explain the moderate liberalism of Prussia’s political professors in the later nineteenth century.

THE SCIENTIFIC PRE-EMINENCE OF GERMAN UNIVERSITIES IN THE NINETEENTH century was due primarily to Wilhelm von Humboldt’s reform of Prussia’s education system, culminating in 1810 in the founding of Berlin university. Humboldt and his successors thought that teaching and research should be united and directed to a common goal—the advance of knowledge through scholarship and science (*Wissenschaft*). They had an idealistic belief that the pursuit of science would not only further knowledge but also uplift the hearts and minds of students and teachers alike. It did not occur to the reformers that the university’s new and ennobling purpose might also require a new organization. The moral effect of the new spirit of science and scholarship was expected to flourish in the *ancien-régime* university structure, with its estate-like hierarchy of full professors (*Ordinarien*), associate professors (*extra Ordinarien*) and lecturers (*Privatdozenten*).⁴ The system was not reformed until the 1930s—a durability that should not suggest timeless perfection.

³ On Lenz, see Hans-Heinz Krill, *Die Ranke-Renaissance: Max Lenz und Erich Marcks—Ein Beitrag zum historisch-politischen Denken in Deutschland, 1880–1935* (Berlin, 1962), 87–88, 105–06, 108–13. On the reform committee, see Max Lenz, *Geschichte der königlichen Friedrich-Wilhelms-Universität zu Berlin*, 4 vols. (Halle, 1910–18), 2, pt. 2: 257–77, esp. 262, n. 1. The committee’s surviving records, “Universitätsangelegenheiten, 1847–1848,” can be found in the Deutsches Zentralarchiv, Abteilung II Merseburg (hereafter DZAM), Rep. 92 v. Gneist, Nr. 22.

⁴ Friedrich Paulsen, *Geschichte des gelehrten Unterrichts auf den Deutschen Schulen und Universitäten vom Ausgang des Mittelalters bis zur Gegenwart*, 2 (1921; reprint ed., Berlin, 1965): 249–51; Fritz K. Ringer, *The Decline of the German Mandarins: The German Academic Community, 1890–1933* (Cambridge, Mass., 1969), 23–25; and Lenz, *Universität zu Berlin*, 2, pt. 1: 186–87, 276–77. For *Wissenschaft*, see Ringer, *Decline of the German Mandarins*, 102–04.

The disorders which disrupted Berlin university between March and August 1848 were the result of long-standing grievances. The junior faculty (associate professors and lecturers) resented the patronizing condescension (*Bevormundung*)⁵ of their superiors and the personal dependence, penury, and exploitation which characterized the indefinite period of their apprenticeship. There was no outlet for such discontent, since the junior faculty had no representation—indeed, almost no rights—in a university run by full professors. Each year from its own ranks senior faculty elected all officials—the rector, five senators, and four faculty deans. These ten men, together with the outgoing rector, formed the university senate. The rector was the chief executive. In consultation with the senate, he administered the university according to instructions given by the minister of education. Similarly, each dean administered his faculty in consultation with its full professors.⁶

The university was a self-administering, but not a self-governing corporation. It had no more autonomy than the minister of education allowed. He had first-hand information about university affairs through two agents who were installed after the Karlsbad decrees (1819): the special government commissioner (*ausserordentlicher Regierungsbevollmächtigter*) and the university judge.⁷ The minister had to confirm the rector and the deans in office, and he alone could make professorial appointments. The faculties conferred only the junior academic rank of instructor (*Privatdozent*) through the *Habilitation*, which entitled the holder to lecture in his specialty.⁸ Above this rank the minister made promotions and appointments on his own authority, and before 1848 he rarely consulted the faculty. He could choose among the many local instructors or appoint external candidates. Every professor bargained individually with the minister over salary and other benefits. Since, contrary to Humboldt's intention, Berlin university did not receive an endowment making it financially independent, the university had little control over its own budget, which was drawn up and administered by the ministry of education. Another of Humboldt's proposals—that professors receive salaries commensurate with their elevated social status⁹—was only partly fulfilled, and great inequities of income developed.

⁵ Strictly translated, *Bevormundung* means "tutelage"; it was a pejorative term used to describe the patronizing, condescending air of government and university officials.

⁶ Lenz, *Universität zu Berlin*, 4: 223–41; and Ringer, *Decline of the German Mandarins*, 36–37. Matthew Arnold, *Higher Schools and Universities in Germany* (London, 1874), 145–66, gives a brief, idealized, contemporary overview.

⁷ On the duties and powers of these agents, see the instructions of November 18, 1819; Johann F. W. Koch, *Die preussischen Universitäten: Eine Sammlung der Verordnungen, welche die Verfassung und Verwaltung dieser Anstalten betreffen*, 2 vols. (Berlin, 1839–40), 1: 16–24.

⁸ The *Habilitation* required at least two years of postdoctoral study, an independent monograph, and a public lecture before the appropriate faculty. See Lenz, *Universität zu Berlin*, 4: 229, 257–58; and Max Weber, *On Universities*, ed. and trans. Edward Shils (Chicago, 1973), 5, n. 3.

⁹ On appointments, see Lenz, *Universität zu Berlin*, 1: 189, 442–43; and Alexander Kluge, *Die Universitäts-Selbstverwaltung: Ihre Geschichte und gegenwärtige Rechtsform* (Frankfurt/M., 1958), 176–77. Also see Ringer, *Decline of the German Mandarins*, 33; and Ewald Horn, "Zur Geschichte der Privatdozenten," *Mitteilungen der Gesellschaft für deutsche Erziehungs- und Schulgeschichte*, 11 (1901); 49, 57. According to Humboldt's plan, associate professors were to be paid 600–800 Thaler, and full professors 1200–1500 Thaler (1 Thaler = 3 Marks). But the university budget was smaller than Humboldt had envisaged and the government had promised; see Lenz, *Universität zu Berlin*, 2, pt. 1: 408–10. In 1840 Berlin university had a budget of about

As the term *Privatdozent* implies, an instructor was not paid a salary but free-lanced; he held private lectures for which he charged fees. The associate professor had to give public lectures on assigned topics; for these he might receive a salary which he could augment by fees from private lectures and tutoring. For the junior faculty private lectures were the main source of income. The full professor had whatever salary he could negotiate, plus lecture and examination fees; if elected rector or dean, he also shared in the substantial fees levied for registration, attendance certificates, and graduation.¹⁰ In theory, everyone taught in a laissez-faire system, competing freely for auditors and charging what fees the market would bear. In fact, the full professors had a marked competitive edge. The deans had to approve the lectures proposed by instructors and used that power to protect full professors from competition; inasmuch as only full professors sat on examination boards, their lectures had special importance; and seniority assured them first choice of lecture rooms, which were in limited supply. In December 1847 this shortage of lecture rooms led a group of the Berlin junior faculty to organize a protest, and out of this group evolved the 1848 reform committee.¹¹

The full income of university teachers cannot be calculated accurately, because no one publicized what he earned in lecture fees. Regular salaries, however, are known. In 1834, when a Berlin student needed about 300 Thaler a year, a full professor's salary averaged 1,203 Thaler (1,050 Thaler in 1850). Twenty professors shared another 11,500 Thaler from administration and graduation fees, but half of this sum was divided among the four deans and the rector. All professors could earn additional income from lecture fees. By comparison, the average salary of an associate professor was 368 Thaler (300 Thaler in 1850), about half of what Humboldt had envisaged.¹² Moreover, no less than one-half of the university's teachers had no salary, just lecture fees. Given an overall student-teacher ratio of about twelve to one, only a few popular, outstanding teachers who drew large numbers of students could live by private lecturing alone. In the mid-1830s the case of Johann Gustav Droysen was not exceptional. An associate professor of history, he lectured

100,000 Thaler, of which 95% came from the state; the figures for 1850 are 171,000 Thaler and 97%; see Lenz, *Universität zu Berlin*, 3: 529–30.

¹⁰ On the history of the lecture fee system, see E. Horn, *Kolleg und Honorar: Ein Beitrag zur Verfassungsgeschichte der deutschen Universitäten* (Munich, 1897), 138–42; and Lenz, *Universität zu Berlin*, 1: 282, 286, n. 2, and 4: 255, 262–63. For the situation in the mid-nineteenth century, see Koch, *Die preussischen Universitäten*, 1: 65–66, and 2: 205–06, 267–72; and “Untersuchungen über das Honorarwesen an deutschen Universitäten,” *Akademische Monatsschrift*, 2 (November, 1850): 479–86, and 3 (January, 1851): 3–22. Also see Ringer, *Decline of the German Mandarins*, 35–38.

¹¹ On the lecture room shortage, see Lenz, *Universität zu Berlin*, 2, pt. 1: 439; and the Circular by the jurists R. Gneist, A. Collmann, and A. Berner, December 29, 1847, DZAM, Rep. 92 v. Gneist, Nr. 22, f. 1. On the deans' rights to censor lectures, see Lenz, *Universität zu Berlin*, 4: 247–48; and Koch, *Die preussischen Universitäten*, 1: 71, 97.

¹² Wilhelm Dieterici, *Geschichtliche und statistische Nachrichten über die Universitäten im preussischen Staate* (Berlin, 1836), 72–74; in 1834 salaries in Berlin ranged from 2500 Thaler to 100 Thaler, and the majority fell between 1500 and 700 Thaler (1 Thaler = 3 Marks). Salary statistics for 1850 are taken from *Stenographische Berichte über Verhandlungen der zweiten Kammer*, 5 (Berlin, 1850): 3140, 3176; the 300 Thaler average for associate professors given there is calculated on the assumption, without foundation, that each one received a salary—how many did is not known. *Stenographische Berichte* gives the total salary budget for 1850 as 78,000 Thaler, while Lenz, *Universität zu Berlin*, 3:529, reports 98,300 Thaler.

ten hours weekly (eight hours privately, two in public), but earned enough only to cover his contribution to the university widows' insurance fund, which he—although a bachelor—was obliged to make. He supported himself by teaching another twenty hours or more in a Gymnasium and by translating, tutoring, and writing. Among others, the philosopher Carl Ludwig Michelet and the physicist Adolf Erman reported similar circumstances in the 1840s. Both stressed how humiliating it was for married men in responsible positions to beg the minister of education for a stipend or to depend on parental support. The jurist Rudolf Gneist, whose lecture income rose from about 600 to 2,000 Thaler between 1840 and 1848, was an exceptional success; his nearest competitors attracted only one-fifth to one-tenth his enrollment.¹³ Of eighty-two teachers who collected fees for private lectures in 1834, fifty-two earned under 200 Thaler, and twenty-four less than 50 Thaler.¹⁴ There were simply too many teachers competing for student fees. German academics in the mid-nineteenth century did not, as a rule, come from wealthy families. The great majority were the sons of civil servants, military officers, small landholders, writers, or artists—groups whose incomes were usually modest and sometimes insecure. Thus the young academic without private means faced uncertain years of penury and drudgery.¹⁵

The situation of junior faculty members was not exceptional for Prussia. The lowest ranks of the state bureaucracy were crowded with university-trained candidates who faced a decade or more of unpaid service. The government could not limit the number of those who passed the *Habilitation* and became university instructors. But it did try to reduce the number of candidates for the civil service by raising examination standards, and it discouraged law students by warning that those entering the judiciary must expect to support themselves for at least eight years. In the early 1840s roughly one-third of the government's legal officials who qualified for a salary were unpaid. John Gillis has suggested that without their services the Prussian judicial system would have ground to a stop.¹⁶ The same was true in universities which, without the help of the junior faculty, could not have kept pace with the great expansion of knowledge.

During the 1840s the condition of Berlin junior faculty deteriorated, not only because student enrollment declined but because government policy changed. King Frederick William IV (1840–1857) believed his mission was to restore the Christian foundations of the state and monarchy and slay the "dragon of

¹³ On Droysen, Erman, and others, see Lenz, *Universität zu Berlin*, 2, pt. 1: 418–24; also see Erman to Eichhorn, December 23, 1840, in Lenz, *Universität zu Berlin*, 4: 569–71; and Carl Ludwig Michelet, *Wahrheit aus meinem Leben* (Berlin, 1884), 79–81, 91, 129, 131. For Gneist's income, see DZAM, Rep. 92 v. Gneist, Nr. 4, 5; and for Gneist's enrollment, see DZAM, Rep. 76 Va. Sekt. 2 Tit. IV, Nr. 45, vol. 1.

¹⁴ Dieterici, *Geschichtliche und statistische Nachrichten*, 74.

¹⁵ Alexander Busch, *Die Geschichte des Privatdozenten: Eine soziologische Studie zur grossbetrieblichen Entwicklung der deutschen Universitäten* (Stuttgart, 1959), 43–44, 110.

¹⁶ Gillis, *Prussian Bureaucracy in Crisis, 1840–1860*, 39–48. Koselleck, in *Preussen zwischen Reform und Revolution*, 439–40, points out the similar fate of junior academics and civil servants. In the judicial service there were about 5000 paid positions and an additional 2500 unsalaried jurists. For their indispensability, see I. Goldschmidt, *Rechtsstudium und Prüfungsordnung: Ein Beitrag zur preussischen und deutschen Rechtsgeschichte* (Stuttgart, 1887), 193–94.

revolution.” To help him “lead back a reluctant world to living Christianity” and combat Hegelianism and rationalism in Prussia’s universities, the king appointed Johann Albrecht Eichhorn as minister of education and religious affairs.¹⁷ Eichhorn brought more zeal than tact to this task, which raised delicate questions of academic freedom. Whenever possible he harassed academics with left-Hegelian or liberal-Protestant views and promoted men of orthodox, pietist, and Christian-conservative outlook. Because of his policy the philosophy instructor Karl Nauwerk, who ridiculed censorship regulations, was forced to resign from the University of Berlin in 1844. During the same year four left-Hegelians—Heinrich Hotho, Wilhelm Vatke, and Agathon and Franz Benary—were forbidden to start a “Critical Journal for Life and Science” that would have discussed everyday religious, political, and cultural events. Eichhorn personally lectured these associate professors like school boys: he told them that they were ignorant of the affairs of church and state and could not presume to discuss public events from a philosophical standpoint that was not compatible with official policy. When the four asked the university’s senate and faculties to defend academic freedom by supporting the journal, university officials were evasive. In 1848, these four became active supporters of the reform committee together with another left-Hegelian, Carl Ludwig Michelet. In 1847 he dared to question Eichhorn’s church policy and was fired without pension rights. Although the dismissal was rescinded by the king two days later, Eichhorn’s action seemed like crude political intimidation. These and similar cases of petty harassment in other universities gained the minister a reputation for exercising humiliating and arbitrary guardianship over intellectual life.¹⁸

Eichhorn’s academic appointments frequently had a faint odor of political patronage. He favored external appointments, creating new positions and filling vacancies without consulting the university on its needs. One notoriously expensive failure was Friedrich Wilhelm von Schelling. King Frederick William hoped that Schelling would show Berliners how to reconcile reason and revelation, science and Christianity. The philosopher held out for the most extravagant terms and, after months of haggling, finally settled for 5,500 Thaler yearly plus fringe benefits—the highest salary paid at Berlin university before 1910. Schelling failed in his ambitious mission and ceased lecturing after less than five years. When Schelling’s extravagant salary became known, Eichhorn’s reputation suffered.¹⁹ In order to secure desirable candidates, the minister also made stand-by appointments until a vacancy appeared. This

¹⁷ On Frederick William IV and Eichhorn, see Franz Schnabel, *Deutsche Geschichte im neunzehnten Jahrhundert*, 4 (Freiburg, 1955): 329–31; [Heinrich von Treitschke] *Treitschke’s History of Germany in the Nineteenth Century*, trans. E. and C. Paul, 6 (1919; reprint ed., New York, 1968): 574–84, esp. 576; and C. Varrentrapp, *Johannes Schulze und das höhere preussische Unterrichtswesen in seiner Zeit* (Leipzig, 1889), 517–33, esp. 522–23.

¹⁸ See Lenz, *Universität zu Berlin*, 2, pt. 2: 73–88, on Nauwerk, and 2, pt. 2: 96–103 on the “Critical Journal.” For Michelet’s account of his tribulations, see his *Wahrheit aus meinem Leben*, 361–74. Also see Lenz, *Universität zu Berlin*, 2, pt. 2: 186–88.

¹⁹ Treitschke, *History of Germany in the Nineteenth Century*, 571–73; and Lenz, *Universität zu Berlin*, 2, pt. 2: 42–56, 368, n. 2. Also see Lenz, *Universität zu Berlin*, 2, pt. 2: 56–65, for evidence of political motives in other appointments.

seemed unfair to junior faculty hoping for promotion. Such appointments led to a steady increase of associate and full professors after 1840 (see graph 1). The simultaneous irregular rise in the numbers of instructors was not Eichhorn's fault; in fact, he proposed ways to restrict it. The university, however, was suspicious of his intentions. Faculties would have adopted his proposal to raise the academic standard of the *Habilitation*, but they opposed as a threat to academic freedom his plans to introduce a merit system and limited-term appointments for instructors, who were to be reviewed every four years.²⁰ While the number of university teachers in all ranks rose, student enrollment began to decline, partly as a result of the depression of the mid-1840s (see graph 2). The student-teacher ratio dropped from twelve to one in 1840 to nine to one in 1847.²¹ The shift is not dramatic. But for many in the junior faculty fewer students meant smaller income and increased frustration—especially since Eichhorn's preference for external candidates reduced the chance for advancement in Berlin.

Before long, Eichhorn heard unprecedented criticism from the university. In August 1843 the philosophy faculty protested that his numerous appointments destroyed the faculty's cohesion, watered down the quality of training in science and scholarship, and forced many professors into "moonlighting." More than forty of seventy-four associate and full professors (not counting medical doctors and academy members) were already working outside the university in order to supplement dwindling lecture fees. Twenty-one associate professors boldly complained to the rector that the minister's external appointments had robbed them of their legitimate hopes for promotion in rank and salary. The rector and senate forwarded this accusation to the minister, declaring in the covering letter that the situation of the junior faculty "deteriorates each year, as almost all hope of a remunerative position fades." Because of the "exaggerated increase" in appointments, an "army of instructors" wanting to become associate professors besieged the ministry, and from their swollen ranks arose laments that there were not enough promotions. The rector added statistics to prove that recently the professoriate had grown too fast, and he asked Eichhorn to consult with each faculty on its needs before making appointments.²² This paltry remedy for a problem affecting more than half of the faculty highlights the university's subordinate status.

In the eyes of the junior faculty, the full professors and the university administration did little to defend the academic freedom and career interests of

²⁰ Lenz, *Universität zu Berlin*, 2, pt. 2: 72–73, 92–93.

²¹ In calculating these figures, which are based on statistics in Rudolf Köpke, *Die Gründung der königlichen Friedrich-Wilhelms-Universität zu Berlin* (Berlin, 1860), 295–99, I did not include the columns "Lektoren und Exerzitienmeister" in Table 3 and "Nichtmatrikulierte Zuhörer" in Table 4. Theodore S. Hamerow, *Restoration, Revolution, Reaction: Economics and Politics in Germany, 1815–1871* (Princeton, 1958), 75–83. According to Knut Borchardt, "The Industrial Revolution in Germany, 1700–1914," in Carlo M. Cipolla, ed., *The Emergence of Industrial Societies*, vol. 4 of *The Fontana Economic History of Europe* (London, 1973), the depression began in agriculture in 1845 (page 99) and spread to heavy industry in 1846 (page 110).

²² Philosophy Faculty to Eichhorn, August 5, 1843; Twenty-one Assistant Professors to the Senate, August 7, 1843; and the Rector and the Senate to Eichhorn, August 16, 1843; Lenz, *Universität zu Berlin*, 2, pt. 2: 70–72, and 4: 589–96.

their younger colleagues. With few resources to fall back on and diminishing expectations, many of the junior faculty came to see themselves, with some justification, as a pool of cheap teachers exploited by a niggardly government so that it could lavish money on professorial stars who brought dubious academic fame to Berlin.²³ But as long as there was a surplus of candidates and a minister who considered political as well as academic qualifications, the junior faculty had to restrain its discontent. In 1848, however, the younger teachers were at last able to speak out more freely and plan a better system.

WHEN THE LEFT-HEGELIAN PHILOSOPHER CARL LUDWIG MICHELET HEARD NEWS of the barricade fighting in Berlin on March 18, he closed his lecture by declaring that henceforth philosophical theory had to be translated into action; it had to “descend into the reality of life and build a free state.”²⁴ The Berlin revolution raised hopes—or fears—that everywhere in Prussian public life authoritarian traditions would yield to more democratic practice. The university students joined Berlin citizens who had demonstrated since early March for an end to absolutism and for civil liberties, constitutional government, and national unity. The reform of the university itself was broached by the musician Adolf Marx and others who called for a meeting of the junior faculty to seek greater academic freedom and a proper voice in university affairs and to mediate between rebellious students and conservative professors. If university officials opposed these demands for representation, the group planned to appeal to the newly appointed minister of education and the general public. When Rector Johannes Müller permitted the unprecedented assembly, he in fact had little choice. Students already occupied the *Aula* for their continuing political rallies. Seventy-three men (three-quarters of the junior faculty) signed up for the March 28 assembly;²⁵ how many attended is unknown.

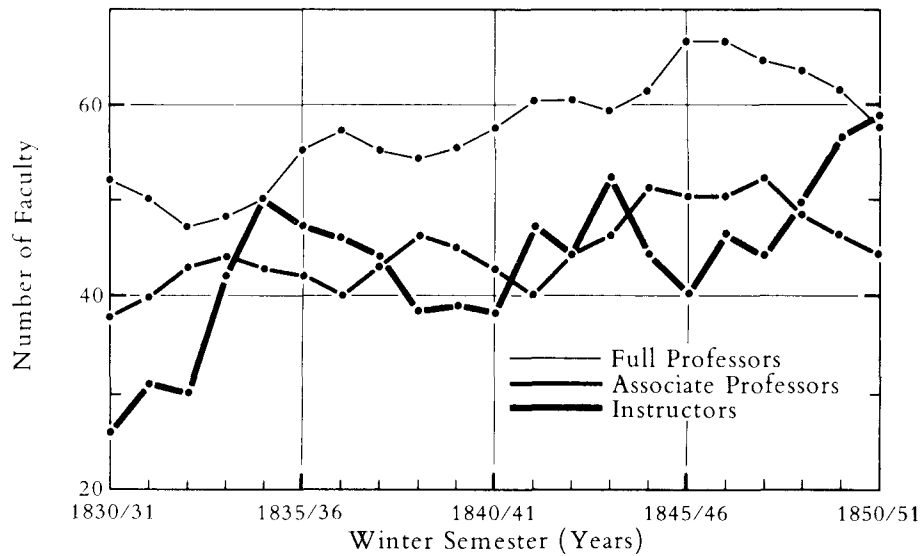
The unfortunate rector Müller, an eminent physiologist, had neither a taste nor a talent for politics. Having permitted the junior faculty’s reform assembly, he tried to outmaneuver it by convening an equally unprecedented meeting of the entire faculty on March 27. Apparently Müller hoped to mobilize conservative forces on the faculty by petitioning the king to recall the former united diet of provincial estates, even though he had already granted Prussia a constitutional parliament to be elected by universal suffrage. On March 27 Müller appeared before the faculty assembly badly prepared. His provocative reactionary petition was still not drafted; yet he prohibited all discussion of its form and content. He had assembled the full faculty for the first time in the university’s history, but he tried to stifle debate. Nevertheless, his proposed petition received overwhelming support (ninety-eight to seven, with two abstentions). The faculty seemed unified in its corporate loyalty to the king and its opposition to the liberal idea of an elected national assembly.

²³ Michelet, *Wahrheit aus meinem Leben*, 132.

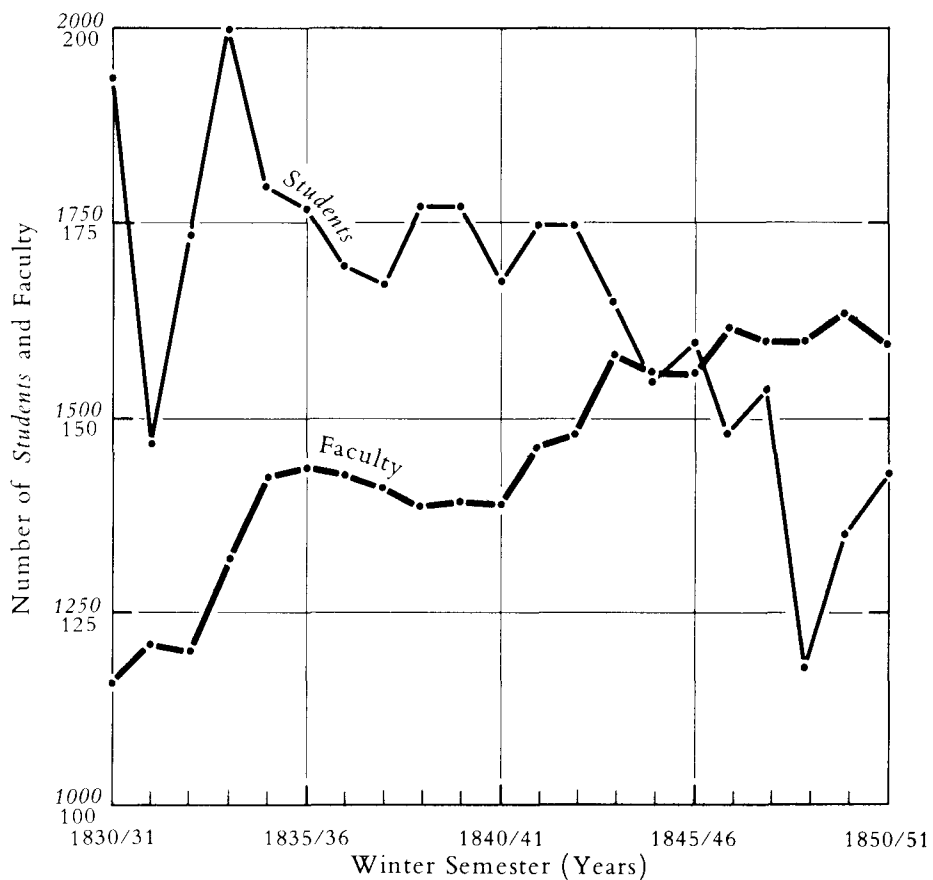
²⁴ *Ibid.*, 163.

²⁵ Lenz, *Universität zu Berlin*, 2, pt. 2: 186–257, recounts student activities in 1848. Circular by A. Marx, March 25, 1848, DZAM, Rep. 92 v. Gneist, Nr. 22, ff. 7, 10. Also see Lenz, *Universität zu Berlin*, 2, pt. 2: 258–59.

BERLIN UNIVERSITY



I. COMPOSITION OF FACULTY



II. GROWTH OF STUDENTS AND FACULTY

Within a day, however, student protests forced Rector Müller to abandon his reactionary resolution.²⁶ Never before had students imposed their will on the university administration, which—like the government—seemed weak and irresolute.

From late March to early August 1848 the junior faculty kept the initiative. A committee of six was elected with Adolf Marx as chairman until late April, when he was replaced by the more forceful but less diplomatic Rudolf Gneist.²⁷ The committee's correspondence with the minister of education reveals the main grievances of the junior faculty and its aspirations. The committee's attempts to achieve recognition and draft a reform program, although they occurred simultaneously, are best described separately.

IN ONE OF ITS FIRST ACTIONS THE COMMITTEE, BACKED BY FIFTY-ONE FOLLOWERS, tried to establish the faculty assembly of March 27 as a precedent for similar meetings in the future. The committee demanded that Müller agree to convene such "advisory assemblies" at the request of any ten faculty members. Since the junior faculty outnumbered the full professors ninety-six to sixty-one, this was the simplest way to end the isolation of the former from and the power monopoly of the latter in university affairs. The committee also demanded that all instructors and associate professors be admitted to and given an advisory vote in meetings of their faculties and that six delegates from the junior faculty be admitted to senate meetings. The rector and senate met this assault upon their prerogatives by passive resistance and an appeal to law. Although he had just summoned one, Müller told the reform committee that he had no authority under university statute to call a general assembly or to admit junior faculty to senate or faculty meetings.²⁸ After several futile exchanges in April and early May relations between the reform committee and the administration deteriorated.

²⁶ On Müller's political skill, see the reports of his eminent pupils Rudolf Virchow, *Johannes Müller: Eine Gedächtnissrede* (Berlin, 1858), 46–48; and [Emil du Bois-Reymond] *Reden von Emil du Bois-Reymond*, ed. Estelle du Bois-Reymond, 2 (Leipzig, 1912): 259–61. For the March 27 assembly, see Lenz, *Universität zu Berlin*, 2, pt. 2: 259; and Michelet, *Wahrheit aus meinem Leben*, 297.

²⁷ DZAM, Rep. 92 v. Gneist, Nr. 22, f. 8, contains the election results. Also see Lenz, *Universität zu Berlin*, 2, pt. 2: 262. The committee members were (1) Adolf Marx (1795–1866), assistant professor (1830) and director (1833) of music; after failing as a jurist, turned to music and pioneered the study of harmonics and composition; not promoted; first chairman of the committee. (2) Rudolf Gneist (1816–1895), instructor (1839) and associate professor (1844) of law, and the most successful teacher in Berlin at the time; became a professor in 1859; succeeded Marx as chairman in late April 1848. (3) Franz Benary (1805–1880), since the early 1830s associate professor for Old Testament exegesis and oriental languages; not promoted. (4) Agathon Benary (1807–1861), since the early 1830s associate professor of classical philology and teacher in the Cöllnische Gymnasium; not promoted; brother of Franz Benary. (5) Heinrich Hotho (1802–1873), lecturer (1827) and associate professor (1829) in aesthetics and art history, and co-editor of Hegel's posthumously collected works; not promoted. (6) Robert Remak (1815–1865), instructor (1847) and associate professor of medicine (1859); the first person of Jewish faith to complete the *Habilitation*, with special permission from the king; in early July left the committee for unknown reasons. And (7) Rudolf Virchow (1821–1902), instructor (1847) in pathology; replaced Remak on the committee. The average age of these junior faculty spokesmen was forty-one.

²⁸ Undated Draft by Gneist (Lenz, *Universität zu Berlin*, 2, pt. 2: 259–60, dates the letter March 30); Rector Müller to A. Marx, April 6 and 25, 1848; the Reform Program of April 15 and Rector Müller to the Committee, May 3, 1848; DZAM, Rep. 92 v. Gneist, Nr. 22, ff. 11–14, 16. Also see Lenz, *Universität zu Berlin*, 2, pt. 2: 261–62.

As planned, the committee then turned for support to the new minister of education, Maximilian Count Schwerin-Putzar, and informed him that the tensions in the university could be overcome only if professors gave up their "exclusiveness" and granted the junior faculty an advisory voice in all university meetings. Clearly, the presence of junior faculty would have ended the confidentiality of senate and faculty meetings. Schwerin showed himself no more willing than the rector to tamper with professorial privilege or university statute. Any amendment required the consent of Prussia's new legislature, to which the minister was accountable.²⁹ Schwerin's reply, however, was not wholly negative. When the federal diet in Frankfurt rescinded the Karlsbad decrees, he instructed each Prussian university to recommend how the powers of the special government commissioner might be redistributed. Schwerin also permitted the junior faculty to elect a small delegation of six associate professors (instructors were deemed ineligible because they were not civil servants) to attend these reform discussions.³⁰ The reform committee's detailed reply to the minister, which the junior faculty assembly approved, shows Gneist's legalistic turn of mind and aggressive tone. Schwerin's offer was rejected as unworthy tokenism: the committee insisted that its request to have nonvoting representatives at faculty and senate meetings did not violate existing law; and it warned that the long task of rewriting the university statute, a document that had fostered "narrow-mindedness, mistrust, selfishness, and intrigue," could never be completed until those who had "a right to be heard" were recognized. (In fact, the junior faculty had never possessed a statutory right to be heard on university affairs.) In closing, the committee declared defiantly that further correspondence with the university administration was useless—it would appeal directly to the cabinet.³¹

Although the committee's letters to Schwerin had lacked traditional deference, its appeal to the cabinet showed even more pent-up hostility and contempt for officials of the university and the ministry of education. Reviewing grievances of the past years, the committee denounced spineless university officials who had condoned Eichhorn's needless and wasteful appointments, his misappropriation of university funds, and his violations of academic freedom. To support these charges, the committee cited the appointment of Schelling, the resignation of Nauwerk, the harassment of Michelet, the prohibition of the Hegelian journal, and other cases. The committee saw the deeper cause of these abuses in the oligarchic university constitution, which permitted a small privileged clique of full professors to rotate in the lucrative university offices, but denied all others a voice in university affairs, even when their teaching responsibilities were the same. The reform committee stressed that represen-

²⁹ The Committee to Schwerin, April 30, 1848, DZAM, Rep. 76 Va. Sekt. 1 Tit. I, Nr. 14, vol. 1; and Schwerin to the Committee, May 3, 1848, DZAM, Rep. 92 v. Gneist, Nr. 22, f. 19v.

³⁰ Schwerin to the Prussian University Commissioners, April 15, 1848; and Schwerin to the Berlin University Rector and Senate, May 3, 1848; DZAM, Rep. 76 Va. Sekt. 1 Tit. I, Nr. 14, vol. 1.

³¹ The Committee to Schwerin, n.d., DZAM, Rep. 76 Va. Sekt. 1 Tit. I, Nr. 14, vol. 1. The letter was approved by a general meeting of the junior faculty on May 17; see the Note by Gneist, DZAM, Rep. 92 v. Gneist, Nr. 22, f. 19.

tation of junior faculty at senate and faculty meetings would not only end the prevailing “narrow-mindedness, intrigue, and nepotism” but foster confidence and understanding as well. Finally, the committee criticized Schwerin for relying on the advice of officials responsible for injustice in the Eichhorn era.³² The charge was partly true, although the committee underestimated the task of a constitutional minister who inherited a tradition-bound bureaucracy with his portfolio.

In early June, Schwerin seemed willing to grant the committee some representation. He suggested that the University of Berlin follow procedures devised at Bonn and consult with representatives of the junior faculty about reform proposals affecting the latter. Schwerin thought the junior faculty “would have in general a freer position” and thereby acknowledged its undue subordination in the past.³³ But the rector and senate felt betrayed by Schwerin and ignored his order until after he left office in late June. His successor, a senior official in the ministry of education, tried to avoid confrontation by encouraging both sides to work out separate reform suggestions.

Somehow the reform committee’s outraged and outrageous letters to Schwerin and to the cabinet were leaked to university officials. They drafted a staunch defense of the traditional prerogatives of full professors against the demands of the junior faculty and against the recommendations for consultation made by Schwerin. The rector and senate declared that in an academic corporation the “majority principle” was valid only among equals in knowledge and scholarship. Thus, instructors were unqualified to share in the rights of full professors, while associate professors had disqualified themselves by joining the instructors and plotting to usurp those rights. The rector and senate urged the minister to resist “the arrogant and egotistical instructors” who had exploited “a momentary weakness of all vested authorities to gain hasty and confused concessions by insolent obstinacy.”³⁴

During these four months the conflict had deteriorated into unsavory name-calling; and no one directly addressed the fundamental question of expanding representation in university affairs. In mid-July after a final flare-up, the correspondence between the reform committee and university officials ceased. The committee, whose position was publicized in the press, saw itself battling an oligarchy of full professors entrenched in university government and enjoying exceptional and unwarranted caste privileges. In turn, the university officials charged that the committee spoke only for a small clique of radicals, allegedly less than twenty percent of the junior faculty, who spread libels in the press.³⁵

³² The Committee to the Cabinet, n.d., DZAM, Rep. 76 Va. Sekt. 1 Tit. I, Nr. 14, vol. 1. The letter was approved by the junior faculty on May 20 and probably sent on May 28. See the Note by Gneist, DZAM, Rep. 92 v. Gneist, Nr. 22, ff. 19, 64; an excerpt is printed in Lenz, *Universität zu Berlin*, 2, pt. 2: 265, n. 2.

³³ Schwerin to the University of Berlin Rector and Senate, June 9, 1848, DZAM, Rep. 76 Va. Sekt. 1 Tit. I, Nr. 14, vol. 1. Friedrich von Betzold, *Geschichte der rheinischen Friedrich-Wilhelms-Universität von der Gründung bis zum Jahr 1870*, (Bonn, 1920): 438. Lenz, *Universität zu Berlin*, 2, pt. 2: 266–69.

³⁴ Lenz, *Universität zu Berlin*, 2, pt. 2: 267–69, printed an excerpt of the fifteen-page document dated June 17 but not sent until June 24, 1848. For the full text, see DZAM, Rep. 76 Va. Sekt. 1 Tit. I, Nr. 14, vol. 1.

³⁵ The Committee to the Rector and Senate, July 10, 1848, DZAM, Rep. 76 Va. Sekt. 1 Tit. I, Nr. 14, vol. 1, and published in *Spener’sche Zeitung*, July 12, 1848. The Rector to the Committee, July 16, 1848, DZAM, Rep. 92 v. Gneist, Nr. 22, f. 50.

BY MID-JULY THE COMMITTEE WAS READY TO PRESENT ITS REFORM PROGRAM TO the junior faculty. But first, to find out how much support it still had, the committee canvassed all 108 instructors and associate professors. Twenty-four were away; of the remaining eighty-four, forty-three pledged their continued support by accepting a membership fee of one Thaler; of these, seventeen also promised to submit reform proposals. This response showed a decline from the more than seventy who had backed the committee in March, or the fifty-odd supporters of early April. In late July support dwindled further. Marx himself resigned from the committee. In a reference to the tone of the committee's correspondence with officials, he blamed Chairman Gneist for "vehement, immoderate outbreaks" that had disrupted "delicate diplomatic maneuvers." For similar reasons thirty more followers withdrew their support, including nine who had agreed to the membership fee only a week before. The committee was left with about thirty-four supporters, or about forty percent of the junior faculty. It had to fear a vote of confidence and at a general meeting on July 23 a motion to force one was barely defeated (fourteen to ten)—a dubious victory, since committee members themselves took part in the vote. Attendance dropped from thirty-four to twenty-three at subsequent meetings held to debate the reform program, partly because the proposals were too radical³⁶ and partly because the summer term was ending.

Gneist opened the general meeting on July 23 with an account of the committee's activities and aims. He repeated the charge that the university had lacked leadership long before March 1848. The intrigue and nepotism which prevailed in its administration could be eliminated only by more openness and a wider franchise in university elections. Everyone agreed that thorough reforms were needed, and a debate on reform proposals was scheduled for July 29. After a long discussion, it was also decided to invite full professors and students.³⁷ On behalf of the full professors, Rector Müller declined the invitation and warned that the presence of students might impede frank discussions of reform.³⁸ The committee ignored his advice.

Max Lenz saw the invitation of the full professors as a revolutionary act akin to the invitation issued by the Third Estate to the upper estates of France in 1789.³⁹ But the comparison is inappropriate. While the *tiers état* claimed to represent the French nation, the Berlin junior faculty never aspired to speak for the whole university. As an unrepresented part of the whole, the junior faculty merely wanted recognition. The committee's reform proposals were, in fact, based on the assumption that the university corporation included everyone, teachers as well as students. Yet, although the junior faculty wanted to

³⁶ For the canvass and the committee's covering letter of July 16, 1848, from which a promise to seek a vote of confidence is left out, see DZAM, Rep. 92 v. Gneist, Nr. 22, ff. 53–56. Also see the Draft Letter, *ibid.*, ff. 39–41. Letter by thirty junior faculty members, July 28, 1848; Marx to Gneist, August 4, 1848; Nonconfidence Motion by G. Curtius, July 23, 1848; and Minutes of the General Meetings, July 29 and August 5, 1848; DZAM, Rep. 92 v. Gneist, Nr. 22, ff. 83–84, 61, 87, 51v, 79, 85.

³⁷ Minutes of the General Meeting, July 23, 1848; and Draft Letter, which outlines the committee's activities, July 23, 1848; DZAM, Rep. 92 v. Gneist, Nr. 22, ff. 79v, 45–47.

³⁸ The Committee to the Rector, July 25, 1848; and the Rector to the Committee, July 26, 1848; DZAM, Rep. 92 v. Gneist, Nr. 22, ff. 67, 62v.

³⁹ Lenz, *Universität zu Berlin*, 2, pt. 2: 269.

emancipate itself and the students at the same time, there is no evidence that the reform committee ever sought an alliance with the students against the rector and the senate; the committee always saw itself as mediator between the administration and the students, though both disregarded its efforts. The sources of the reform program are uncertain. One possible source was a letter written to Gneist in early April by the botany lecturer Wilhelm Walpers, who suggested that ministerial interference in the university be minimized, that professorial privileges be trimmed, and that the junior faculty be given representation. These points were in the committee's first list of reform proposals published in mid-April. But Walpers had made other demands which the committee did not adopt: to eliminate the intermediary rank of associate professor and to prohibit full professors from accumulating remunerative offices.⁴⁰ Presumably he was attacking the practice of dividing graduation and examination fees among the rector and deans.

The committee's April program had envisaged a three-phase reform. First, the junior faculty would be admitted to all faculty meetings, including degree examinations, but only with an advisory vote. Second, after appropriate legislation, it would gain a full vote. At this stage the antiquarian use of Latin required in examinations, theses, and public ceremonies would end, and students might receive some voice in electing the rector. Last, in the third phase the reform discussion would turn to general issues—for example, whether or not the university should place scholarship above practical instruction and how the university should be related to the state. Profound changes were called for: an end to direct government supervision; a guarantee of tenure, salary, and pension rights for university teachers in all ranks; and an end to all privileges and perquisites enjoyed by full professors. When these suggestions were drafted in mid-April, the committee still hoped to work out the details in cooperation with the full professors.⁴¹ By July, when this hope had vanished, the committee came up with a streamlined reform program that was to be implemented immediately.

The introduction to this program declared that the development of "German science and scholarship" (*Wissenschaft*) had to be the foremost concern of any university reform. Essential conditions for the growth of scholarship were "free competition in teaching, unrestricted influx of younger and more vigorous people, and elimination of all external constraints on teachers and students."⁴² The claim that scholarship could flourish only in *laissez-faire* academic competition protected the junior faculty against the charge that the

⁴⁰ Walpers to Gneist, April 4, 1848, in Lenz, *Universität zu Berlin*, 2, pt. 2: 269; also printed partly in Veit Valentin, *Geschichte der Deutschen Revolution von 1848–49*, 2 (1931; reprint ed., Aalen, 1968): 577. It is strange that Valentin's study reveals so little interest in university reform activities in 1848. As an instructor he himself was harassed into resigning in 1917 after a bizarre dispute with his senior colleague Georg von Below; see *Biographisches Wörterbuch zur deutschen Geschichte*, 3 (Munich, 1975): 2975; and Ringer, *Decline of the German Mandarins*, 56–57.

⁴¹ DZAM, Rep. 92 v. Gneist, Nr. 22, f. 12v. The program was approved by the general meetings of April 15 and 17, 1848 before it was submitted to the senate; see Lenz, *Universität zu Berlin*, 2, pt. 2: 260–62.

⁴² DZAM, Rep. 92 v. Gneist, Nr. 22, f. 27. The eighteen-page manuscript has corrections by Gneist which correspond to the decisions taken at the general meetings of July 29 and August 5, 1848; see *ibid.*, ff. 27–37.

dismantling of professorial privilege was inspired by selfish motives. The body of the July program consisted of six points, the first of which dealt with the fundamental question of university-state relations by demanding the abolition of the special government commissioner, the office which for three decades had symbolized government supervision (*Bevormundung*) of academic life. Yet the committee would not concede full autonomy to the universities. In the interest of scholarship the universities needed supervision to counteract abuses common to all corporations: "exclusiveness, selfishness, laxity, and excessive indulgence of professional colleagues." The committee therefore wanted to resurrect in modified form the pre-1819 university trustee (*Kurator*), who had been responsible for the proper administration of the university. But the committee insisted that the office be separated from that of the senior provincial official (*Oberpräsident*), who had held it before 1819 in addition to his other duties, and entrusted to a lesser official, whose sole task would be day-to-day university administration and the protection of state interests. He would curb "the petty desire for administration" to which academics were prone, although, as "higher intellects" they rarely possessed the requisite talent. "The real burdens of administrative responsibility are detrimental to the actual calling of a university teacher; it involved scholars [*Männer der Wissenschaft*] in the petty arts of administrative intrigue and nepotism, increasing in equal measure their dependence on superiors and patronage towards inferiors."⁴³ Supervisory authority was to be vested in a career bureaucrat in order to protect academics from the corrupting effect of power.

Reviving the trustee was not merely a tactical concession designed to make the other reform proposals more acceptable to the government. When the idea was challenged in the general meeting of July 29, those who favored government supervision of universities won a clear majority over supporters of university autonomy. In that meeting Michelet and others argued that the proposed trustee contradicted the trend toward "self-government [*Selbstregierung*]" for which scholars are most qualified."⁴⁴ Michelet had just submitted a lengthy memorandum on educational reform to Gneist. There he had asked rhetorically, "If we really want to go from the patronizing police state [*Polizei-Staat*] to self-government [*Selbstregierung*] and the strict adherence to public law [*Rechtsstaat*], can there be any higher realm of freedom than the absolute autonomy and autarchy of science?" Consequently, he advocated a completely democratic university: an assembly composed of all university teachers would elect the rector, deans, and senators, although candidates for the first two offices had to be full professors. In Michelet's plan academic appointments and promotions were apparently to be decided in each faculty by all professors and instructors.⁴⁵ But he failed to persuade the meeting of the benefits of democratic self-government. The majority (fourteen to six) sided

⁴³ *Ibid.*, ff. 28–29 v.

⁴⁴ *Ibid.*, ff. 79–80.

⁴⁵ Michelet to Gneist, July 26, 1848, *ibid.*, ff. 68–78. The fifteen-page manuscript is not complete. C. L. Michelet, *Vorschläge zur Umgestaltung der deutschen Universitäten* (Berlin, 1850), 8–11, 14–21; also see Michelet, *Wahrheit aus meinem Leben*, 330–33.

with Gneist, who insisted that self-government be “restricted to science and scholarship,” meaning unspecified academic matters.⁴⁶ The second point in the reform program of July would have safeguarded the university from undue governmental interference by replacing the university judge, who exercised the corporation’s judicial authority, with a permanent legal advisor, who was to defend the university’s statutory rights vis-à-vis the state-appointed trustee. Another point, not extant in Gneist’s papers, dealt with tenure and presumably restricted the education minister’s right to dismiss instructors.⁴⁷

The remaining three points were to rid the university of outdated regulations and to broaden participation in its government. Consistent with the effort of the revolutionary regime to subject all Prussian citizens to the same laws and courts, the university was to give up its judicial rights over students, except in disputes involving questions of money or honor. Latin was no longer to be compulsory at official university functions; French and German were to be allowed in dissertations and degree examinations.⁴⁸ But the most far-reaching proposal concerned elections to university offices now designated as “honorary” (*Ehrenämter*)—a term that implied an end to the days when a small group of officials divided the lucrative administration and graduation fees. The reformers believed it was self-evident that scholarship could not be apolitical. Indeed, “a university teacher had the duty to take a public stand on political questions. Now, as formerly, the university should have a political role and, we hope, it will not be negligible, but in accordance with the university’s status.” Every university office had political importance and consequently had to be filled “by an election process that conforms to the political creed of the state.” In the election of the rector, the spokesman for the whole university, political considerations outweighed all others, including even administrative ability. He needed the broadest democratic mandate, so that student confidence in the university could be restored. Therefore, the committee recommended that the entire student body should elect the rector. But this was too daring for the junior faculty assembly, which decided that the rector should be elected by the entire teaching staff and an equal number of student representatives. In each faculty, students would elect the dean, although teachers were to nominate candidates. Lastly, the senators, who had mostly administrative duties, would be elected by all professors and instructors.⁴⁹

This plan certainly broke up the “guildlike exclusiveness” of the university’s governing body, the eleven-man senate, which would no longer consist of full professors chosen by their peers. Six senators (the rector, the outgoing rector, and four deans) were to be elected by students, the remaining five by all professors and instructors. Most students would have been able to vote in university elections earlier than in state elections, where the minimum voting

⁴⁶ DZAM, Rep. 92 v. Gneist, Nr. 22, f. 79 v.

⁴⁷ *Ibid.*, ff. 30–31, 85.

⁴⁸ *Ibid.*, ff. 35–37.

⁴⁹ *Ibid.*, ff. 33–34 v. Unfortunately, there is no record of how many students attended these meetings or of what they thought about the role assigned to them.

age was twenty-four.⁵⁰ Although the disruptive potential of this scheme to democratize the university was obvious, the committee claimed its plan would end the "cold segregation" of students and teachers and establish a "living bond" between them. Possible objections by the full professors were brushed aside. "We cannot understand," the committee wrote, "that the dignity of a full professor could be injured if his election is based on the vote of confidence of several hundred educated youths. Experience teaches . . . that youth is incorruptible, and whatever it [youth] may lack in the sureness of academic judgment is compensated for by its keen incorruptible gaze into the heart and character of any individual teacher."⁵¹ Very few full professors would have subscribed to such romantic notions about the infallible judgment of untutored youths. In its boundless faith in the mediating powers of elections, the committee failed to recognize that democracy works only if the parties involved agree to accept the election results. The full professors had left no doubt that it was beneath their dignity to discuss university affairs in the presence of nonvoting deputies of the junior faculty. Most would have regarded a rectorship bestowed by student mandate as Frederick William regarded the German emperorship offered by the Frankfurt parliament—as "a dog collar to chain me to the revolution."⁵² The committee's electoral reform plan could not have pacified the university.

The reform of university governance at Berlin would never have survived the collapse of the German revolution. But it was denied even this honorable end by the failure of the reformers themselves. On August 8, three days after the final meeting of the junior faculty, Gneist left to do research in England. He kept the program in his papers. No one else took the initiative to keep the reform in motion (it was between semesters and the university was not in session). Shortly after Gneist's return in mid-October, a military coup closed the door to liberal reform in Prussia. Thereafter, committee members tried to forget their activities. Gneist's academic career stagnated for a decade, at least in part because of his reform zeal. In 1849 Virchow, then a "*persona minus grata*" in Berlin, was dismissed for "constant and organized opposition to the government" but later reinstated on a reduced income. Shortly afterward he accepted a professorship in Bavaria. The academic careers of the other committee members also seem to have languished.⁵³

Most reform programs drafted by junior faculty committees in other Prussian and German universities were more moderate than that of Berlin. Student participation in university elections was also demanded in Breslau. In

⁵⁰ Ernst Rudolf Huber, *Deutsche Verfassungsgeschichte seit 1789*, 2 (Stuttgart, 1960): 584.

⁵¹ DZAM, Rep. 92 v. Gneist, Nr. 22, f. 33 v.

⁵² Rudolf Stadelmann, *Soziale und politische Geschichte der Revolution von 1848* (1948; reprint ed., Darmstadt, 1962), 136.

⁵³ On Gneist's career, see Erich J. C. Hahn, "Rudolf von Gneist (1816–1895): The Political Ideas and Political Activity of a Prussian Liberal in the Bismarck Period" (Ph.D. dissertation, Yale University, 1971), 52–56. For Rudolf Virchow, see his *Briefe an seine Eltern, 1838–1864*, ed. Marie Rabl (Leipzig, 1907), 170–74; and for the unusual estimate of Virchow's status, see Michelet, *Wahrheit aus meinem Leben*, 132. Gneist, Virchow, and Michelet were all discreetly silent about their university reform activities. Rudolf Gneist, *Berliner Zustände: Politische Skizzen aus der Zeit vom 18. März 1848 bis zum 18. März 1849* (Berlin, 1849), 127 n; Virchow, *Johannes Müller*, 46–49; and Michelet, *Wahrheit aus meinem Leben*, 325–31.

Jena a more radical group called for an autonomous democratic university in which associate and full professors would have the same rights, and officials would be elected by an assembly of all students and teachers.⁵⁴ The Jena program was drafted for a national congress on university reform that convened in September 1848. Prussian universities were prohibited from sending delegates to the Jena congress, which debated most of the issues raised in Berlin and elsewhere. Since fully half of the Jena delegates were full professors, the final resolutions were more conservative than the reform demands of most junior faculties. Nevertheless, one observer denounced the congress as a meeting of job malcontents (*Stellungs-Malkontente*).⁵⁵ By a narrow margin the delegates passed a motion to give university government a broader base through regular advisory meetings of the *corpus academicum*, an assembly of all university teachers which would also elect university officials. Thus the congress chose to abolish oligarchical rule by professors. The controversial question of university-state relations was postponed for a later congress, which never met.⁵⁶ The national effort for university reform succeeded no better than did the Berlin program.

What occurred in the universities in 1848 was not unique in the Prussian civil service. Except for the church and the army, all branches of the state service experienced protests from junior staff members against incomes and subaltern status. The army officer corps could gain nothing by revolution and was from the start more royalist than the indecisive king. Churchmen adhered to the Lutheran tradition that the powers-that-be were instituted by divine authority and could not be challenged.⁵⁷ In the judiciary and administration some younger officials entered politics hoping to abolish the authoritarian state. As parliamentarians they placed the demand for political reform and national unity, both necessary prerequisites for the liberalization of society, above narrow professional aspirations. A few even regarded themselves as part of an intellectual proletariat and sought solidarity with the exploited working man.⁵⁸ After 1848, some purges and administrative reprisals removed or silenced formerly rebellious officials, and the government restored its control over the civil service.⁵⁹ Hence the experience of German professors and

⁵⁴ For Breslau, May 11, 1848, see DZAM, Rep. 76 Va Sekt. 1 Tit. I, Nr. 14, vol. 1; for Halle, March 29, April 11 and 25, 1848, see *ibid.*; and *Deutsche Universitäts-Zeitung*, 1 (1848-49): 44-46, 53-56. For Bonn, see Betzold, *Geschichte der rheinische Friedrich-Wilhelms-Universität*, 437-38; Hans Kersken, *Stadt und Universität Bonn in den Revolutionsjahren 1848-49* (Bonn, 1931), 114-23; and *Deutsche Universitäts-Zeitung*, 1 (1848-49): 11-12. For Königsberg, see *Deutsche Universitäts-Zeitung*, 1 (1848-49): 221-24, 227-29. For Jena, see Karl Griewank, *Deutsche Studenten und Universitäten in der Revolution von 1848* (Weimar, 1948), 58-59. For Tübingen, see Eberhard Sieber, *Stadt und Universität Tübingen in der Revolution von 1848-49* (Tübingen, 1975), 39-47. An attempt to raise the question of university reform in the Prussian national assembly failed; see Norbert Andernach, *Der Einfluss der Parteien auf das Hochschulwesen in Preussen, 1848-1918* (Göttingen, 1972), 4-8.

⁵⁵ Busch, *Die Geschichte des Privatdozenten*, 55.

⁵⁶ Griewank, *Deutsche Studenten und Universitäten*, 60-77, esp. 75; *Deutsche Universitäts-Zeitung*, 1 (1848-49): 2-4; and *Offizielle Protokolle über die Verhandlungen deutscher Universitätslehrer zur Reform der deutschen Hochschulen in Jena vom 21. bis 24. September, 1848* (Jena, n.d.), 22-29, 34.

⁵⁷ M.-K. Messerschmidt, "Die preussische Armee während der Revolution in Berlin, 1848," *Revue Internationale d'Histoire Militaire*, 32 (1972-75): 466-69. Fritz Fischer, "Der deutsche Protestantismus und die Politik im 19. Jahrhundert," *Historische Zeitschrift*, 171 (1951): 485-87.

⁵⁸ Gillis, *Prussian Bureaucracy in Crisis, 1840-1860*, 97-98, 104-10.

⁵⁹ *Ibid.*, 149-60.

universities in 1848 mirrored that of the bureaucracy and administration as a whole.

THE BERLIN UNIVERSITY REFORM PROGRAM OF 1848 CONTAINED NOTABLE INCONSISTENCIES. It proclaimed that the university had to provide leadership in public affairs but denied it full autonomy by reviving the governmental watchdog, the trustee. This inconsistency can partially be explained by the seeming success of the Prussian revolution through mid-summer 1848, when the program was debated. It appeared that future ministers of education would be constitutionally responsible in some way to an elected parliament,⁶⁰ which could have blocked violations of academic freedom like the Karlsbad decrees or administrative tutelage à la Eichhorn. A university trustee who was accountable to a minister who was in turn accountable to a new democratic parliament could not, it was thought, become an instrument of oppression. But the decision to revive the trustee was based only in part on mistaken confidence in the success of the revolution. For these liberal academics, the trustee was a characteristic compromise between their desire for corporate autonomy and their respect for state authority. In university affairs as well as in public life Prussian liberals wanted participation but not full responsibility, self-administration (*Selbstverwaltung*) but not self-government (*Selbstregierung*).⁶¹

The notion of self-administration rests on the idealized Hegelian view of the state as neutral arbiter above the competing interest groups in civil society.⁶² Every state institution had to be shielded against conflict of interest arising from the general frailty of human nature. Therefore the reform plan of the Berlin committee, which contained three left-Hegelians (Hotho and the brothers Benary) and one sympathizer (Gneist), was a model of university self-administration: the government trustee did not have enough authority to indulge in the condescending guardianship of an absolutist bureaucracy, but he could keep in check "exclusiveness, selfishness, laxity, and favoritism." In the Revolution of 1848 the debate on the reform of Prussian local government shows a parallel dispute. The advocates of self-administration, who defended state supervision of local institutions prevailed over those who believed that local and regional autonomy, or self-government, was an essential condition of a democratic system.⁶³ The reform of Prussian local government was rescinded after 1851. Later, during the 1860s, Gneist achieved fame as an advocate of authoritarian (*obrigkeitlich*) self-administration by which he meant a large degree of bureaucratic supervision of elected local assemblies. Not surprisingly, the man who had deemed university teachers incapable of

⁶⁰ See Erich Hahn, "Ministerial Responsibility and Impeachment in Prussia, 1848-1863," *Central European History*, 10 (1977): 1-27.

⁶¹ Heinrich Heffter, *Die deutsche Selbstverwaltung im 19. Jahrhundert: Geschichte der Ideen und Institutionen* (Stuttgart, 1930), 264-67.

⁶² Franz Rosenzweig, *Hegel und der Staat* (1920; reprint ed., Aalen, 1962), 118-20, 137-42. Shlomo Avineri, *Hegel's Theory of the Modern State* (Cambridge, 1972), 141-47, 176-84.

⁶³ Heffter, *Die deutsche Selbstverwaltung im 19. Jahrhundert*, 310-13, 315-18.

exercising corporate autonomy also thought that Prussian subjects should not run their own county and provincial affairs.⁶⁴

The Berlin reform program, moreover, ignored the idealistic view that *Wissenschaft* perfects the individual as it leads him to truth. The program did, however, associate scholarship directly with the responsibility to shape public life; that academics had to “side with a party” openly “by word and deed” seems to have been undisputed. Michelet saw in the revolution an historic opportunity to translate philosophy into political action; the democrat Virchow wrote on May 1, “As a natural scientist I can only be a republican.” He was certain that no other form of government could meet the needs of human nature; the most he could concede was a hereditary president—“that is, a monarch without authority.”⁶⁵ Virchow stood to the left of the constitutional monarchist Gneist in 1848. Under Bismarck both men became leading parliamentarians and political professors, though the pathologist Virchow doubtless found less political inspiration and utility in scholarship than Gneist, the pamphleteering jurist and political scientist. Their confidence that politics and scholarship could be harmoniously united was rarer among academics by the end of the nineteenth century. The practical needs of the Wilhelmine Empire, an industrial world power, tended to make the idealistic assumptions of *Wissenschaft*, as formulated by Humboldt and others in the early nineteenth century, seem redundant or untrue. As a consequence, German academics became not only self-doubting, but also self-justifying, defensive, and backward-looking.⁶⁶

When Friedrich Meinecke recalled his years as a *Privatdozent* in Berlin during the 1890s, he wrote, “The mentality of the ambitious and thwarted university instructor is well known. But one hears of it more often in autobiographies, correspondence, et cetera of the later nineteenth century than in those of the earlier part—which does not speak well for the later period. It means a certain externalization in bearing one’s fate of which I probably was not free either.”⁶⁷ Meinecke idealized the stoic bearing of his predecessors for the wrong reasons. They were probably not, as he implied, morally superior. Their insecure position and modest hopes commanded them to be silent about humiliations and disappointments, to repress anxieties resulting from personal dependence and indefinite penury. Normally, when education ministers selected their candidates in a marketplace that knew no shortage, political dependability mattered as much as scholarly achievement. Junior faculty members in Prussia and elsewhere spoke out only once—in the political spring of 1848. The widespread demands for university reform testify to longstanding and deep dissatisfactions: many members of the junior faculty regarded themselves as exploited apprentices and saw the full professors clinging to anachronistic “parchment prerogatives.”⁶⁸

⁶⁴ Hahn, “Rudolf von Gneist (1816–1895),” 161–64, 176–79, 190–91.

⁶⁵ *Briefe an seine Eltern*, 142, 145.

⁶⁶ Ringer, *Decline of the German Mandarins*, chaps. 5, 6.

⁶⁷ Friedrich Meinecke, *Erlebtes, 1862–1901* (Leipzig 1941), 215–16. I am grateful to Wilma Iggers for this translation.

⁶⁸ Michelet used this term in his reform memorandum, DZAM, Rep. 92 v. Gneist, Nr. 22, f. 71.

The 1848 university reform controversy has broader significance. The willingness of the German liberal professors to compromise with government authority has often been documented but never fully explained. Leonard Krieger has pointed out that the ideas of German liberals were "doubly conditioned," by the political, economic, and social circumstances within Germany and by the personal circumstances of the individual thinkers.⁶⁹ The aspiring instructor or associate professor who was not independently wealthy faced an indefinite period of academic and financial insecurity. His professional apprenticeship—a protracted minority without guarantees of scholarly and financial reward—fostered conformism and tended to discourage the independent-minded from pursuing an academic career. Given these circumstances of academic conditioning or socialization, it is hardly surprising that the German professor, having passed through the purgatory of the instructor and associate professor ranks, developed a lasting respect for tradition and authority.

⁶⁹ Krieger, *The German Idea of Freedom* (Boston, 1957), x.

J. B. Bury's Philosophy of History: A Reappraisal

DORIS S. GOLDSTEIN

GENERAL TREATMENTS OF LATE NINETEENTH- AND EARLY TWENTIETH-CENTURY European intellectual history have usually excluded Britain, stating (or implicitly assuming) that the British did not take part in those developments which were radically altering Western European thought.¹ In no field does this cleavage between Britain and the Continent appear to be sharper than in historiography and historical theory. British historians seem not to have taken part in, and indeed to have been totally isolated from, those theoretical and practical innovations associated with the names of Henri Berr, Karl Lamprecht, Max Weber, and Benedetto Croce.² Certainly until the work of R. G. Collingwood in the postwar years, British scholarship made no original contribution to speculation about the nature and scope of historical knowledge. Perhaps the explanation lies, as both Herbert Butterfield and E. L. Woodward have suggested, in a congenital antipathy to abstract thought about such problems.³ Yet, in the period before 1914 there was one British historian—J. B. Bury—who did venture into the thickets of historical methodology and epistemology.

Bury was a prolific writer, ranging through ancient, medieval, and modern history in his various books, contributions to scholarly journals, and critical editions. Today, fifty years after his death in 1927, he continues to be known as the author of *The Idea of Progress* and of narrative histories of the later Roman and Byzantine empires. But it is his inaugural address as Regius Professor of Modern History at Cambridge, delivered in January 1903, that has won for Bury an enduring, if somewhat dubious, notoriety. That lecture, "The Science of History," described the transformation of historical studies during the nineteenth century and pointed out its momentous consequences. Briefly

I should like to express my thanks to Felix Gilbert and Georg G. Iggers for their careful reading of this essay and their helpful comments and suggestions.

¹ See, for example, H. Stuart Hughes, *Consciousness and Society* (New York, 1958), 13. For a recent criticism of this point of view, see Reba N. Soffer, "The Revolution in English Social Thought, 1880–1914," *AHR*, 75 (1970): 1938–64.

² That British historians were not involved in these historiographical discussions and controversies has been pointed out by Felix Gilbert, "Intellectual History: Its Aims and Methods," *Daedalus*, 100 (1971): 96, n. 15.

³ Butterfield, *Man on His Past* (Boston, 1960), 22–23; and Woodward, "Some Considerations on the Present State of Historical Studies," Raleigh Lecture on History (London, 1950), 95.

stated, Bury held that, as a result of the rise of the critical method in Germany and the new understanding of history as a developmental process, a qualitative change in the discipline had taken place: "Girded with new strength she [history] has definitely come out from among her old associates, moral philosophy and rhetoric; she has come out into a place of liberty, and has begun to enter into closer relations with the sciences which deal objectively with the facts of the universe."⁴ Hence, "history is not a branch of literature." Nor is its function the didactic one of expounding moral and political lessons. Almost at the beginning of the address Bury asserted, "It has not yet become superfluous to insist that history is a science, no less and no more." The final sentence reiterated the theme that, "though she [history] may supply material for literary art or philosophical speculation, she is herself simply a science, no less and no more."⁵

Leaving aside for the moment the question of whether or not these statements have been misconstrued, the point is that they have largely determined the image of Bury held by historians. The burden of contemporary response to the lecture, and especially of G. M. Trevelyan's well-known rebuttal, was that Bury had succumbed to the "Germanic" tendency to reduce history to the bloodless facts and abstractions of the library and the laboratory.⁶ This "Professor Dry-as-dust" stereotype has remained; and among those of a more philosophic turn of mind Bury's insistence that history is a science has been taken to mean that he shared the assumptions of nineteenth-century positivism or scientism.⁷ Such perceptions have served to impede serious consideration of Bury's theoretical preoccupations and, hence, to preclude proper understanding of the inaugural address itself. Bury, moreover, spoke to similar questions concerning the nature, methods, and aims of historical scholarship in other lectures and essays and in scattered passages in his books. Only if we examine Bury's premise—history is a science—within this larger framework can we clarify his statement and comprehend his meaning. For Bury addressed himself to problems that have since been given more concrete delineation by those who have come to be called the analytic philosophers of history. Although inconclusive in its debates about the nature of historical explanation, the analytic school has provided the logical tools with which to dissect the thought of a historian like Bury.

⁴ "The Science of History" (1903), in *Selected Essays*, ed. H. Temperley (Cambridge, 1930), 11.

⁵ *Ibid.*, 4, 8–9, 11, 22.

⁶ S. H. Butcher, *Harvard Lectures on the Originality of Greece* (London, 1920), 251–55; John Morley, review of Frederic Harrison's *Thucydides: The Crusade of the Tenth Century* in *The Nineteenth Century*, Oct. 1904, pp. 575–76; and G. M. Trevelyan, "The Latest View of History," *Independent Review*, 1 (1903): 395–414.

⁷ Sir Charles Firth, *A Commentary on Macaulay's History of England* (London, 1938), 29–30; C. V. Wedgwood, *Truth and Opinion* (London, 1960), 62, 89–90; and E. L. Woodward, *British Historians* (London, 1943), 46, are among those who have perpetuated the stereotype. For Bury as positivist, see chiefly R. G. Collingwood, *The Idea of History* (Oxford, 1946), 147–51, and also Fritz Stern, *The Varieties of History* (New York, 1956), 20. It should be noted, however, that Arthur Marwick has presented more sophisticated comments about Bury's supposed scientism in *The Nature of History* (London, 1971), esp. 77–78. In addition, a recent article by a classical scholar, although not concerned with analysis of Bury's historical theory, does call "for an assessment of his place in the history of scholarship": George Huxley, "The Historical Scholarship of John Bagnell Bury," *Greek, Roman, and Byzantine Studies*, 17 (1976): 104.

IN DEALING WITH THAT ENTIRE CLUSTER OF PROBLEMS CENTERING ON CAUSAL explanation in history, Bury's initial endeavor was to contrast the modern concept of history as a "causal process," which "contains within itself the explanation of the development of man from his primitive state to the point which he has reached," with the older notion that history was determined from without, by the will of God. Only when history was perceived as wholly subject to the laws of cause and effect, he pointed out, could it become a science.⁸ Such a position is unexceptionable, though scarcely illuminating, for it is merely to say that historians assume the possibility of natural rather than supernatural explanation. The crucial problem remains: can history, thus conceived as a "causal process," be made to yield uniformities and, possibly, causal laws, or is a sympathetic understanding of sets of unique events and processes the proper definition of the historian's task? Typically, Bury approached this problem of the relative merits of the nomological and hermeneutic conceptions of historical explanation with judiciousness and impartiality.⁹ For an Auguste Comte or a Henry Thomas Buckle historical study should establish laws which will describe the causal uniformities of whole aggregates or classes of historical phenomena; for those who reject generalization and prediction, historical study should "trace in detail a singular causal sequence."¹⁰ Efforts at generalization, Bury noted, are more fruitful in economic history and *Kulturgeschichte*, where aggregate behavior is of greater importance. Within the body of work which endeavored to subsume historical data under general laws, Bury singled out Lamprecht's *Die kulturhistorische Methode* as "the ablest product of the sociological school of historians."¹¹ There can be no question about his interest in, and understanding of, the goals of the nomological approach: he was not inclined to dismiss out of hand its claim to add a new dimension to historical knowledge. But Bury stated categorically that historical generalizations were not laws, that they could provide no basis for deduction or prediction, and that such generalizations therefore had only a heuristic value: neither the role of individuals nor that of coincidence could be contained within any nexus of causal uniformities. Chance and individual action limit the scope of generalization and, therefore, make deduction and prediction impossible.¹² Like the proponents of the hermeneutic method, then, Bury stressed both the unique occurrences and the human volition inherent in the historical process. Although this emphasis meant rejecting the search for deductive and predictive laws, it did not preclude the possibility of causal explanations at some level of generality. In effect, he chose to occupy a middle position, urging historians to give "serious

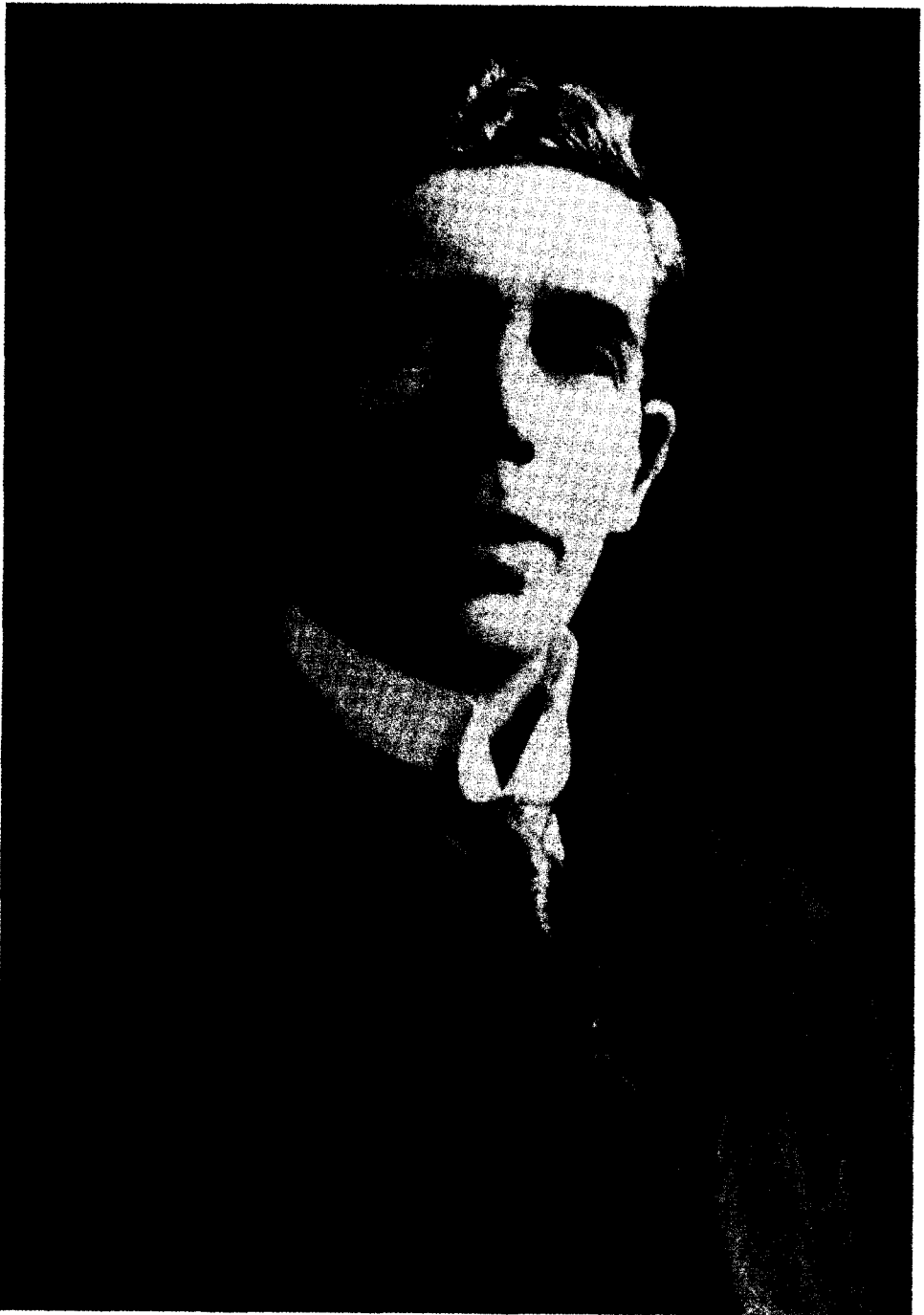
⁸ "Darwinism and History" (1909), in *Selected Essays*, 26-27; also see 24.

⁹ For a discussion of the development of these two conceptions of historical explanation, see Georg Iggers, *New Directions in European Historiography* (Middletown, Conn., 1975), chap. 1.

¹⁰ "Darwinism and History," 29-30, 33; also see Bury, "The Place of Modern History in the Perspective of Knowledge" (1904), in *Selected Essays*, 44-46.

¹¹ "Darwinism and History," 41; also see 38.

¹² *Ibid.*, 38-42. Bury elsewhere remarked, "For the comprehension of history we have perhaps gained as little from Comte's positive laws as from Hegel's metaphysical categories"; *The Idea of Progress* (New York, 1955), 301.



J. B. Bury in early life. Photo courtesy of University Library, Cambridge, and Cambridge University Press: H. Temperley, ed., *Selected Essays of J. B. Bury* (1930).

thought” to theories like those of Lamprecht, while nevertheless recognizing that they were applicable only to certain types of historical phenomena.¹³

The aspect of historical explanation that perhaps interested Bury most was that of contingency. Aware of the importance of what he termed the “chapter of accidents” in history, he offered his own interpretation of how historical contingency should be understood. Chance as it operated in history was not “the intrusion of a lawless element”; he defined it instead as “the valuable collision of two or more independent chains of causes—‘valuable’ meaning that it is attended with more or less important consequences.”¹⁴ The accidental was not therefore outside the realm of cause and effect, but was rather the product of a causal sequence quite unconnected with the historical situation it influenced.¹⁵ For purposes of further clarification Bury introduced the categories of “pure” and “mixed” contingencies. The distinction between the two can best be explained by using Bury’s own illustration. If Napoleon had been killed by a meteorite, the situation would have been a “pure” contingency, since “the meteorite was completely disinterested in his death.” If, on the other hand, Napoleon had been killed by an enemy for political reasons, this would have been a “mixed” contingency, “for the assassin was interested in Napoleon’s death, and the causal sequence which led him to commit the act would have been connected with the causal sequence which rendered the great man’s death historically important.”¹⁶

In the “Cleopatra’s Nose” essay, in which Bury dealt solely with the question of contingency, he proposed a logical structure for the study of the operations of contingency within specific historical situations. His initial premise was that all historical phenomena—those which are defined as “accidental” as well as those which are the result of human purpose—are subject to the law of causation. That is, all occurrences have antecedents. It follows that the causal sequence determining a chance event is as open to discovery and explication as an event stemming from human choice and action. For the historian, according to Bury, “a systematic study of contingencies is a necessary preliminary to any speculations which aim at historical synthesis.” The importance of accidental factors will vary according to the circumstances: “in some cases they produce a situation to which the antecedent situation does not logically lead. In others they determine the form and means of the realisation of a logical tendency.” By “logical tendency” Bury meant the entire complex which at any given historical moment has resulted from “the experience and knowledge of man. . . . It is to this that the historical process owes its logic, so far as it is logical.” He suggested that the role of chance in historical development might diminish as a consequence of an increase in human knowledge of both the natural and social environment.¹⁷

¹³ “Darwinism and History,” 42.

¹⁴ “Cleopatra’s Nose” (1916), in *Selected Essays*, 61. The same definition can be found in “Darwinism and History,” 37 n., and *The Idea of Progress*, 304.

¹⁵ Patrick Gardiner has aptly paraphrased Bury’s point: “when a historian refers to something as having happened by chance, he implies that its explanation lies . . . off the main track of his enquiry or concern”; “Causation in History,” *Dictionary of the History of Ideas*, 1 (New York, 1968): 283.

¹⁶ “Cleopatra’s Nose,” 67–68.

¹⁷ *Ibid.*, 67, 66, 68, 69.

Bury's analysis of contingency has been subjected to varying interpretations and alternately praised and condemned. E. H. Carr has cited with approval Bury's definition of a chance event as one with an independent causal sequence colliding with other causal sequences. According to Carr, Bury's stress on the importance of accident in history should be understood sociologically. That emphasis was related to "the growth of a mood of uncertainty and apprehension which set in with the present century and became marked after 1914. The first British historian to sound this note after a long interval appears to have been Bury."¹⁸ Whether Bury's stance was indeed a reflection of a changing mentality or merely the result of his own reflections on history is moot. Norman Baynes and Harold Temperley, both of whom knew Bury, mention his growing conviction about the significance of contingency, but offer no explanation of how he arrived at this conviction.¹⁹ Herbert Butterfield has remarked that "Bury, after having been too rigid in his initial scientific assumptions, came to the conclusion that the shape of Cleopatra's nose altered the course of history."²⁰ Butterfield implied that excessive belief in the possibility of general explanations led to subsequent despair about the likelihood of such explanations. The central question raised by these comments is whether or not Bury came to think that contingency alone was decisive in historical development. Certainly the "Cleopatra's Nose" essay does not justify this conclusion. Here he referred specifically to "the experience and knowledge of man" creating "a logical situation at a given moment of history" and to the interplay of this "logical situation" and contingencies.

For both Baynes and Temperley, Bury's changing analysis of the causes of the fall of the Roman Empire in the West provides evidence of a drastic shift in his views. In his 1889 *History of the Later Roman Empire from Arcadius to Irene*, he had listed various general causes which led to "decline and fall"; in his 1923 *History of the Later Roman Empire from the Death of Theodosius I to the Death of Justinian*, he stated that "general considerations" could not explain the collapse of the Empire in the West.²¹ Without doubt there is a difference of emphasis in the two works. Nevertheless, to say that "general considerations" cannot explain a particular historical phenomenon is not to assert that there are no general, but only contingent, causes of that phenomenon. What is at issue is the logical distinction between necessary and sufficient cause, the former defined as "a causal component without which an effect will not occur," the latter as "all the other causal components which are required to make it occur."²² Bury's aim in the 1923 *History* was to point out that, despite the general causes (that is, necessary causes) of weakness within the Empire, its dissolution was not inevitable and therefore had to be explained by a series of contingent events (that is, by sufficient causes). The earlier work, then,

¹⁸ Carr, *What Is History?* (New York, 1964), 130–31.

¹⁹ Baynes, *A Bibliography of the Works of J. B. Bury, with a Memoir* (Cambridge, 1929), 72–76, and Temperley, Introduction, *Selected Essays*, xxiv–xxv.

²⁰ Butterfield, *Christianity and History* (New York, 1949), 110.

²¹ *A History of the Later Roman Empire from Arcadius to Irene* (London, 1889), 1: chap. 3; *History of the Later Roman Empire from the Death of Theodosius I to the Death of Justinian* (New York, 1958), 303–13.

²² These succinct definitions are taken from David Hackett Fischer, *Historians' Fallacies* (New York, 1970), 172.

differed from the later one in the respective weight allotted to specific necessary and sufficient causes. As Temperley admits, however, this difference was an alteration imposed by "new materials and fresh interpretations."²³ It does not in itself constitute proof of a radical change in Bury's theoretical views, particularly since his other writings provide no evidence that he became convinced of the primacy of contingency in historical explanation.²⁴

The two chief philosophical critiques of Bury's ideas about contingency are those of Michael Oakeshott and R. G. Collingwood. Although there are significant differences between the two, both were written from the standpoint of philosophical idealism. Hence, both attempt to refute the notion that a properly historical treatment of the past will use the category of cause and argue that causality is a concept applicable only to the natural sciences. Collingwood and Oakeshott found Bury's views of interest precisely because of what they understood to be his growing skepticism about the adequacy of causal explanation in history and his subsequent emphasis on the role of contingency.²⁵ Oakeshott explained that Bury attempted to construct a theory in which the dynamic of historical change could be understood as the interaction of causal sequences and accidental occurrences. Although he did subject this theory to a rigorous analysis colored by his own philosophical assumptions, Oakeshott, in general, faithfully described its substance and intent.²⁶

Collingwood's interpretation, on the other hand, contains grave distortions—distortions which, unfortunately, have helped to shape the received wisdom about Bury's historiographical speculations. According to Collingwood, Bury began by accepting two aspects of the positivistic view of history: that the task of the historian was the gathering of ascertainable, isolated facts and that an event was to be treated "not as unique but as an instance of a certain type, and the explanation of it [was to be found] by discovering a cause applicable not to it alone but to every event of the same general kind."²⁷ Gradually, however, Bury became disenchanted with this position, experienced a "crisis in his thought" in which he attempted to shake off his positivistic past, and was ultimately unsuccessful.²⁸ For Collingwood, the very

²³ Temperley, Introduction, *Selected Essays*, xix.

²⁴ For example, comparison of Bury's *Quarterly Review* article of 1900 on "Causes of the Survival of the Roman Empire in the East" (reprinted in *Selected Essays*, 231–42) with his Introduction to *The Eastern Roman Empire* (vol. 4 of the *Cambridge Medieval History* [Cambridge, 1923], vii–xiv) reveals the same effort to balance the role of general and contingent factors.

²⁵ Collingwood, *The Idea of History*, 176–77, 214–15, 148–50; and Oakeshott, *Experience and Its Modes* (Cambridge, 1933), 132–33.

²⁶ Oakeshott, *Experience and Its Modes*, 140–41. As William H. Dray has pointed out, however, Oakeshott's critique rests in part on a misapprehension of what Bury meant by "cause" and "general cause"; "Michael Oakeshott's Theory of History" in Preston King and B. C. Parekh, eds., *Politics and Experience: Essays Presented to Michael Oakeshott* (Cambridge, 1968), 39–40.

²⁷ Collingwood, *The Idea of History*, 148; see also Collingwood's review of *Selected Essays*, in *English Historical Review*, 46 (1931): 461–62.

²⁸ Collingwood's review of *Selected Essays*, 465. Essentially the same interpretation has been advanced in a recent doctoral dissertation: Arthur Haberman, "J. B. Bury: A Crisis of Historical and Individual Conscience" (Ph.D. dissertation, New York University, 1966). According to Haberman, however, Bury was an inconsistent positivist during the period up to and including the inaugural address of 1903.

essence of this "crisis" was Bury's assertion of the importance of contingency, since it implied rejection of the search for general laws and recognition of the significance of individuality in history. Yet Bury was unable to move further in this direction because of his "positivistic prejudice that individuality as such is unintelligible, and that in consequence the generalizations of science are the only possible form of knowledge."²⁹

Collingwood's summary bears little resemblance to Bury's formulations, largely because the latter have been forced into the rigid mold of a theory of history in which "idealism" and "positivism" relentlessly confront each other. The reader of Collingwood, for example, will search Bury's writings in vain for any approach to the covering-law model of historical explanation, with its assumption that events should be understood as "instances" to be subsumed under general causal laws.³⁰ Collingwood has conflated Bury's belief that causality and causal sequences were legitimate and essential explanatory devices with the positivist search for laws which will describe and predict the behavior of whole classes of historical phenomena. Nor is it accurate to counterpoise an early Bury, neglectful of chance and individuality in his quest for generalizing laws, with a post-"crisis" Bury, who believed that insofar as history was determined by accident and individuality it was unintelligible. By stressing contingency Bury meant to point out the existence of differing types of causal relationships, not to proclaim the reign of the goddess Fortuna. According to Bury's theory of contingency, even chance events were not in the strict sense unintelligible, since they too could be explained in terms of cause and effect. And certainly he never considered individuality itself to be unintelligible unless mediated through, and subsumed under, broad causal explanations. In short, Collingwood read Bury through the lenses of his own idealism and arrived at an interpretation that misconstrued the latter's ideas and that obscured his real concerns.

More generally, both the idealist critique and the view that Bury came to think of contingency as determinative in historical change are misleading. Despite the allegations of Oakeshott and Collingwood, Bury's approach to the problems of historical explanation was not in the positivist tradition, unless this tradition is defined so broadly as to be emptied of content. In discussing contingency Bury sought not to make a priori judgments about the role of chance in history but to suggest that historians examine more carefully the nature and dynamics of accidental causes within specific historical contexts. He wished to call attention to contingency as one type of historical causation and to the necessity of making distinctions among varying kinds of causal factors. Morton White has best caught Bury's intent: Bury illustrated "the

²⁹ Collingwood, *The Idea of History*, 150; also see his review of *Selected Essays*, 464.

³⁰ Collingwood adduces as evidence only Bury's 1900 article entitled "Causes of the Survival of the Roman Empire in the East," where, according to Collingwood, "the survival of the Eastern Empire is regarded as an event of a certain general kind, and the problem is to find causes of certain general kinds to account for it"; review of *Selected Essays*, 462. But in this essay Bury attempts nothing like an analysis of the causes of the survival of empires. His focus is fixed firmly upon the Eastern Empire, with comparisons between the Eastern and Western Empires used as a means of illustration and not of generalization. See "Causes of the Survival of the Roman Empire in the East," 231-42.

correct view that causes are not distinguished merely on the basis of a categorical criterion according to which they must be either states or events. They may be either."³¹ And, because the historian deals with both "states" and "events," with the general and the individual, neither the nomological nor the hermeneutic methods of historical explanation could be rejected out of hand. Precisely because Bury's mode of thought was that of a working historian rather than that of an adherent of any speculative system, he did not pronounce upon the intrinsic validity of these methods but instead defined the prospective uses and limitations of each. Considering Bury's reputation as a positivist, it is ironic that he found those aspects of the nomological approach most closely associated with positivism unacceptable: the emphasis upon predictability and the existence of what Maurice Mandelbaum has called "directional laws." Yet Bury refused to regard the hermeneutic method as sacrosanct. He stated that it was necessary to move beyond the collection of "particular facts" to a consideration of theories which attempted to explain the relationships between individuals and the collective social environment.³² Such theories would be based upon the study of aggregates and uniformities and would therefore result in a more generalized level of causal explanation. Bury was receptive to these potential developments insofar as their aim was not the discovery of laws but the explication of specific historical structures. From a philosophical point of view, his refusal to limit the possibilities of historical explanation to the precepts of any one speculative system or method may be condemned as a vacuous eclecticism. But, despite—or perhaps because of—its lack of theoretical rigor, this posture does correspond to the experience of the historian, confronted as he is by the variety and complexity of historical phenomena. In any event, Bury was among those historians who, before 1914, realized the need for a reconsideration of the methods and goals of historical explanation.

BURY'S "RESTLESS AND FERTILE MIND," TO QUOTE ARNALDO MOMIGLIANO'S TELLING phrase, led him to ponder another thorny question that besets the philosophically inclined historian: what is the nature of historical cognition? Is it inevitably subjective and, if so, in what sense can history be considered a science? Bury's earliest reflections on these questions can be found in a short article, "Anima Naturaliter Pagana," published in 1891. In this essay he argued that it is impossible to recover the pagan past, whatever one's resources of knowledge and empathy. The modes of thought and feeling in the historian's environment cannot be discarded, and these present an insuperable obstacle to the apprehension of alien cultures.³³ This slight but charming essay reveals Bury in a very different mood from that of the sober

³¹ White, *Foundations of Historical Knowledge* (New York, 1965), 138. By "states" White means standing conditions, general considerations, or what the historian frequently refers to as underlying causes.

³² "Darwinism and History," 42.

³³ "Anima Naturaliter Pagana: A Quest of the Imagination," *Fortnightly Review*, new ser., 41 (1891): 102–12.

scholar, patiently accumulating data which he believes will result in a valid reconstruction of the past. Imaginative understanding, rather than causal explanation, is the keynote here. But it would be a mistake to lend too much emphasis to the Crocean implications of Bury's awareness that the historian brings his own present to the study of the past. For Bury hoped that this limitation might be surmounted in the future, that "a new method of historical psychology" would make it possible to trace "the gradual growth through the ages of various emotions and their delicate modifications." This is suggestive of Karl Lamprecht rather than of Benedetto Croce: the historian's imprisonment in his own mental and emotional milieu is not necessarily a permanent obstacle in the pursuit of historical knowledge, but one which might be surmounted by the progress of scholarship.³⁴

To turn to the inaugural lecture is to observe Bury in his chosen role of spokesman and advocate of "the science of history." With a note of triumph, of elation, he described how a new and higher standard of historical truth had emerged in the nineteenth century as a result of the critical method. He went so far as to assert that, when Ranke's dictum—"Ich will nur sagen wie es eigentlich gewesen ist" [*sic*]—has been thoroughly impressed upon historians, "there will no longer be divers schools of history."³⁵ The inaugural address, then, would seem to furnish clear-cut evidence that Bury entertained no doubts about the possibility of reaching complete understanding of the past. Yet this is not the case. A somewhat different view emerged in the course of his discussion of the idea of development, which he considered to have been of equal importance with the critical method in raising history to the status of a science. Here Bury forcefully argued that "the patient drudgery," "the microscopic research," which historians undertake is justifiable because it helps "to build, firm and solid, some of the countless stairs by which men of distant ages may mount to a height unattainable by us and have a vision of history which we cannot win, standing on our lower slope." In one sense, Bury here merely claimed that scholarship is cumulative. But he was also prepared to accept the logical consequence of his emphasis upon the evolving—and therefore relative—nature of historical truth and to recognize a less than absolute value in the historical works produced by each epoch:

It may be said that like the serpents of the Egyptian enchanter they are perpetually swallowed up by those of the more potent magicians of the next generation; but—apart from the fact that they contribute themselves to the power of the enchantment which overcomes them—it is also true that though they may lose their relative value, they abide as milestones of human progress; they belong to the documents which mirror the form and feature of their age, and may be part of the most valuable material at the disposal of posterity.³⁶

³⁴ *Ibid.*, 112. It is unlikely, though possible, that as early as 1891 Bury would allude to Lamprecht's views. The latter's *Deutsche Geschichte* began to appear in 1891, and his first methodological essay appeared in 1896. The two historians probably met in 1904, when both gave addresses at the St. Louis Congress of Arts and Sciences. Bury's first explicit reference to Lamprecht's work occurs in "Darwinism and History" (1909).

³⁵ "The Science of History," 10; also see 5–6.

³⁶ *Ibid.*, 17–18.

As an example he cited Heinrich von Treitschke's German history, which was not "the ultimate history of Germany in the nineteenth century," but rather "a characteristic document of its epoch."³⁷ Again, one should not exaggerate the presentist connotations of this passage; by applying the concept of development to historiography itself Bury meant to stress the growth of historical knowledge, not its subjective nature. He was not denying the possibility of knowing "wie es eigentlich gewesen," but merely deferring that moment of illumination into the future. But the dark side of the moon remains: until it has been illuminated, the historian is condemned to produce works which are, in an absolute sense, valuable only as documents of his own age.

In writings and lectures that followed upon the inaugural address, Bury returned to the problem of historical cognition. While he reiterated that all historical writing reflects the historian's present, he now made an explicit distinction between fact and interpretation: "Dates, names, documents, can be considered purely objective facts," but "the reconstruction, which involves the discovery of causes and motives, which it is the historian's business to attempt, depends on subjective elements." And these "subjective elements" reflect not only the historian's personal experience but changes in collective mentality which provide new formulas for historical interpretation.³⁸ Bury spelled out these ideas most clearly in *The Ancient Greek Historians*, in the course of his explanation of how "historical science" has come to realize that all ideas and events must be understood in terms of their historical contexts and "merely represent a particular stage of human development." In the same way the interpretations of the historian are "conditioned by the mentality of his own age," and therefore he cannot hope to make final judgments. Bury declared sharply and unequivocally that it was a still prevalent "illusion" to think that "a historical judgment may be the last word." The "permanent interest" of historical judgments, he concluded, was that they mirrored the attitudes of their own eras.³⁹ What is absent in these pages is that bracing belief—present in the inaugural address—that sometime in the future historical truth will emerge from the cumulative labors of historians. And yet, despite his acknowledgment of the fragile and changing nature of historical knowledge, Bury continued to insist upon the scientific status of history. Indeed, in *The Ancient Greek Historians* these concepts are linked. History had become a science precisely because of its recognition of the genetic or developmental principle; and, when this idea is applied to historiography, its logical corollary is the relativity of all historical interpretation.

In what sense, then, did Bury consider history to be a science? Most obviously, as he himself noted, his usage of the word "science" was meant to correspond to *Wissenschaft*.⁴⁰ Historical scholarship was that branch of *Wissenschaft* which accumulated and presented facts about the past in an orderly

³⁷ *Ibid.*, 18.

³⁸ Bury, Introduction to Edward Gibbon, *The Decline and Fall of the Roman Empire* (6th ed., London, 1912), 1: xiv. Also see "The Place of Modern History in the Perspective of Knowledge," 51.

³⁹ *The Ancient Greek Historians* (New York, 1938), 250, 252–53.

⁴⁰ *The Life of St. Patrick* (London, 1903), viii n.

and comprehensible fashion. The reconstruction of facts became possible with the establishment of the critical method; order and comprehensibility became possible once the genetic (or developmental) approach to historical phenomena was accepted. This transformation of history into *Wissenschaft* did not, however, imply facile analogies with the natural sciences. Bury warned against such analogies and explicitly rejected the view that history was merely a "higher zoology."⁴¹ But, although the distinctive nature of its subject imposed upon historical research methods and problems different from those of the natural sciences, the historian need not limit himself to a narrow and mindless empiricism. The effort to achieve meaningful and coherent syntheses, as well as respect for fact, was the common obligation of all *wissenschaftliche* activity. The "science of history" came into existence when it shouldered these responsibilities. Thus, "fact-gathering" was not an end in itself: "I cannot imagine the slightest theoretical importance in a collection of facts or sequences of facts, unless they mean something in terms of reason, unless we can hope to determine their vital connection in the whole system of reality."⁴²

The tone as well as the content of this statement is revealing. Unlike many of his contemporaries who struggled with the problem of historical cognition, Bury seems not to have been afflicted with what Marc Bloch has described as "a disillusioned humility."⁴³ Bloch was referring to those whom Henri Berr caustically dubbed "*historiens historisants*," and specifically to Charles Seignobos. Bury's distinction between fact and interpretation is analogous to Seignobos' discrimination between analytic and synthetic history: both attempted to preserve some vestige of epistemological realism while recognizing the relativistic nature of historical knowledge.⁴⁴ Whether or not this position is indeed philosophically naive, as the Croce-Collingwood tradition would insist, is not at issue; for Seignobos, acceptance of what would now be called "perspectivism" resulted in a sense of unease or futility, which is perhaps best exemplified in his well-known remark, "Facts which we do not see, described in language which does not permit us to represent them in our minds with exactness, form the data of history."⁴⁵ Possibly the key to Bury's very different attitude lies in the fact that he was, in a manner of speaking, a "convert" to historical scholarship. Because he came to history from classical philology, Bury was intensely conscious of the advances that had been made possible in the former by the application of the critical methods of the latter. Whatever the methodological limitations, he was not prone to denigrate or minimize the

⁴¹ "The Place of Modern History in the Perspective of Knowledge," 46-47. Also see "Darwinism and History," 28 n., 36; and "The Science of History," 5-11.

⁴² "The Place of Modern History," 47.

⁴³ Bloch, *The Historian's Craft* (New York, 1953), 15.

⁴⁴ There is no evidence that Bury was familiar with Seignobos' ideas. Bury's library did not contain any of Seignobos' methodological or theoretical writings, though it did have a copy of the Bernheim *Lehrbuch der historischen Methode*; see *Catalogue of the Library of the Late J. B. Bury*, Blackwell's Catalogue # 228 (Oxford, 1927/28).

⁴⁵ Charles Langlois and Charles Seignobos, *Introduction to the Study of History*, tr. G. G. Berry (London, 1898), 221. Seignobos wrote this section of the book.

successful reconstruction of historical fact. More important, he considered the genetic or developmental idea as one which had revolutionized—and liberated—the study of history. Indeed, his insistence upon this point is reminiscent of nothing so much as Friedrich Meinecke's affirmation that "the rise of historicism was one of the greatest intellectual revolutions that has ever taken place in Western thought." For Bury wholeheartedly accepted the historicist point of view, with its implicit relativism and consequent overthrow of "the illusion of finality."⁴⁶ Because he identified the transformation of history into *Wissenschaft* with the genetic or historicist approach, he was able to regard perspectivism not as the negation, but as the mirror image, of the scientific status of history. Then again, Bury's temperament, free from a thirst for absolutes, permitted him to acknowledge the relative nature of historical knowledge without dismay.

Bury's views on historical cognition strike the same note as his ideas on historical explanation: neither are distinguished by theoretical rigor but rather by an eclecticism that reflects the concerns and experience of a practicing historian. Hence he rejected some of the claims of both epistemological realism and idealism in favor of a middle position which holds that facts, although existing independently of the historian's mind, are chosen and interpreted according to the perspective of the historian. It can be argued that Bury's adherence to this position demonstrated a rather sophisticated awareness that the pristine forms of realism and idealism offer inadequate means of resolving the problem of historical cognition. Harriet Gilliam has recently spoken of the "tension" or "dialogue" between realism and idealism which "takes the form of a series of efforts, often quite deliberate, to accommodate and reconcile the views of one school to those of the other."⁴⁷ If this is indeed the case, then Bury's own effort at reconciliation becomes significant for the evaluation of his place in the development of twentieth-century historiographical theory.

Whatever the merit of Bury's reflections about historical explanation and historical cognition, they are internally consistent, unlike his views about whether or not history should be written without personal animus or partiality. Should the historian, in his writing of history, openly display his *parti pris*? Not only was Bury obscure and at times inconsistent on this question, but he also tended to conflate the question of impartiality with that of history as literature. This is evident, for example, in the introduction to his edition of Edward Gibbon's *Decline and Fall of the Roman Empire*. Here Bury pointed out that, if Gibbon were writing his history in the nineteenth century, his attitude would be one of "detachment," since he would have imbibed the "historical

⁴⁶ Warren Wagar, in *Good Tidings* (Bloomington, Ind., 1972), 188, recognizes Bury's "implicit avowal of the premises of historicism" in the epilogue to *The Idea of Progress*. Obviously I am not using the term "historicism" in Karl Popper's sense, but defining it as a concept which stresses individuality, development, and relativism. These have recently been suggested by Pietro Rossi as the "three typical elements of historicism" in "The Ideological Valences of Twentieth-Century Historicism," *History and Theory*, Beiheft 14, *Essays on Historicism*, 14 (1975): 16.

⁴⁷ Gilliam, "The Dialectics of Realism and Idealism in Modern Historiographic Theory," *History and Theory*, 15 (1976): 231.

point of view."⁴⁸ Yet Bury himself is curiously reticent about affirming "detachment" as a virtue in historical writing, and this reticence is linked to the issue of literary merit. He noted more than once that Gibbon's prejudices lend interest and color to the narrative; and Bury commented, "it has sometimes been remarked that those histories are most readable which are written to prove a thesis."⁴⁹ After giving examples of historians who wrote with a "party spirit" (Theodore Mommsen, George Grote, Johann Droysen) as against those who maintained an attitude of impartiality (Leopold von Ranke, Mandell Creighton), he did not take a stand in favor of either position. At the end of his introduction, he ventured the somewhat sibylline remark that "it might be reasonably maintained that zeal for men or causes is an historian's mar-ring, and that 'reserve sympathy' . . . is the first lesson he has to learn," but concluded by observing that Gibbon's "zeal," or "zealous distrust of zeal," was "an essential and most suggestive characteristic of the *Decline and Fall*."⁵⁰ Bury thus implies that Gibbon's very prejudices were "essential" to the writing of his masterpiece and that this very subjectivity is "suggestive" of the role of *parti pris* in the creation of great historical works.

At first glance, it appears difficult to reconcile this somewhat indecisive stance with the authoritative manner in which, in the inaugural address, Bury swept aside the notion of any integral connection between history and literature. In "The Science of History" Bury celebrated the newly won freedom of history from literature as well as from moral philosophy. Nevertheless, the significance of Bury's pronouncement that "history is not a branch of literature" should not be exaggerated, since he went on to concede that "the facts of history . . . can supply material for literary art." His cardinal point was that "to clothe the story of a human society in a literary dress is no more the part of a historian as a historian, than it is the part of an astronomer as an astronomer to present in an artistic shape the story of the stars."⁵¹ Plainly, he wished to assert as sharply as possible the distinction between historical research itself ("the science of history") and the presentation of the results of that research in literary form. Bury's delineation of such a distinction does not resolve the issue: should a historian, in the name of vivid and provocative narrative, cast aside detachment and openly write as a partisan? Despite appearances, then, and the invocation of the Rankean "wie es eigentlich gewesen," the inaugural lecture does not serve to explicate Bury's views on this subject.

Much of the criticism of the address ignored Bury's distinction between historical research and the casting of that research into aesthetically pleasing narrative. The phrase "history is a science, no less and no more" was taken to

⁴⁸ Introduction to Gibbon, *The Decline and Fall of the Roman Empire*, 1: xxix; also see the Introduction to Gibbon's *Autobiography* (London, 1907), where Bury stated that Gibbon did not have what nineteenth-century historians were to acquire, namely, "historical sense"—to judge an age by its own ideas and ideals" (xiv).

⁴⁹ Introduction to *The Decline and Fall of the Roman Empire*, 1: xli; also see xxxix–xl, lxvii–lxviii.

⁵⁰ *Ibid.*, lxviii.

⁵¹ "The Science of History," 9.

mean that the new regius professor wished to exclude the literary element from historical writing.⁵² Bury responded to this accusation in the "Preface" to his *Life of St. Patrick* (1905). He explained that he "never meant to suggest a proposition so indefensible as that the presentation of the results of historical research is not an art, requiring tact and skill in selection and arrangement which belong to the literary faculty"; and he also made explicit a position which he had endeavored to set forth in the inaugural lecture by stating that the text of the *Life of St. Patrick* was "an effort in the art of historiography," while the appendices to the book "represent the work which belongs to the science of history."⁵³ This was an effective rebuttal, especially to Trevelyan's remark that Bury wanted history to be presented "without any literary dress,"⁵⁴ but one has a sense that despite the minor furor occasioned by the inaugural address the significant questions were neither asked nor answered. For example, there is the obvious question of whether or not historical research can be separated so tidily from "the art of historiography." Because his critics focused upon his supposed denigration of history as literature, this question was neglected, together with that of the relationship between "the art of historiography" and historical bias. Bury's attempt to sever research from literary presentation was analogous to his epistemological distinction between fact and interpretation. But here Bury lacked the willingness to think through the implications of his position with the rigor he displayed in dealing with the epistemological problem. Having exonerated himself from the charge of regarding historical writing as synonymous with "the science of history," he explored the matter no further.

In 1926, however, less than a year before his death, he wrote a curious letter to the London *Morning Post* on the subject of impartiality in the writing of history. The tone of this letter is utterly different from that of most of Bury's work. He discarded his usual cautiousness and discretion, and he forthrightly pronounced, "I do not think that freedom from bias is possible, and I do not think it is desirable. Whoever writes completely free from bias will produce a colourless and dull work." He argued that "the most effective histories"—for example those of Tacitus, Gibbon, Mommsen, and Thomas Babington Macaulay—have exhibited a *parti pris*, because the writer must be personally involved in order to produce "instructive" history. Only if the historian is interested in his subject, and therefore has an opinion about it, can his work interest others.⁵⁵ To explain this letter Baynes commented that Bury must have undergone a "change of outlook" from the position he had set forth in the *Life of St. Patrick*.⁵⁶ Certainly the priority which Bury accorded to interesting and "instructive" historical writing in this letter is not encountered

⁵² S. H. Butcher, *Harvard Lectures on the Originality of Greece*, 251–55; Morley, review of Frederic Harrison's *Theophrastus*, 575–76; and G. M. Trevelyan, "The Latest View of History," *Independent Review*, 1 (1903): 395–403, 412–14.

⁵³ *The Life of St. Patrick*, viii, viii n.

⁵⁴ Trevelyan, "The Latest View of History," 395.

⁵⁵ "A Letter on the Writing of History" (1926), in *Selected Essays*, 70–71.

⁵⁶ Baynes, *A Bibliography of the Works of J. B. Bury, with a Memoir*, 105.

elsewhere in his work. Logically, however, there is no inconsistency, since in the *Life of St. Patrick* he had sharply separated historical research from "the art of historiography," with the implication that the former constituted the realm of objective fact and the latter that of personal and subjective historical narration. The 1926 letter assumes this distinction, and in it Bury finally stated explicitly that impartiality is not desirable in the art of historical writing.

What is puzzling, if not jarring, in Bury's conclusion here is his negation, in effect, of his own lifework. For above all detachment and lack of bias characterize most of Bury's historical writings. Only in the *History of Freedom of Thought* (1913), a popular work meant for the edification of the British public, did he write from a partisan point of view.⁵⁷ Perhaps August Heisenberg, a fellow Byzantinist, has given the best description of Bury's work: "His verdicts on people and things are objective, sober, and severe, fairly and acutely weighted, but reserved. As a historian, he chose, like Ranke, only to narrate the way things were. Therefore he limited himself strictly to that which the sources handed down to us. . . ."⁵⁸ Nor was Bury's unwillingness to indulge in rhetoric and the expression of personal judgments confined to his historical writing: as a lecturer at Cambridge and within the British historical profession he was regarded as the prototype of an objective and impartial scholar. According to Temperley, Bury felt it to be "a duty . . . to avoid giving his personal impressions of a historical problem."⁵⁹ His Cambridge lectures on "The Use of Authorities" corroborate Temperley's testimony. They are careful, detailed, and judicious, but they provide few hints as to Bury's own views.⁶⁰ There is, then, a radical disjuncture between the entire thrust of his work and the sentiments expressed in the 1926 letter. Nor can this letter be dismissed as merely a passing and insignificant vagary on Bury's part, since it is not inconsistent with a point of view for which he had previously shown some sympathy. What remains inexplicable is that he toyed with, and ultimately accepted, a set of opinions about impartiality and "the art of historiography" which were utterly at variance with his practice as a historian.

Perhaps the key to the enigma lies in Bury's effort to accommodate himself to the British historiographical tradition, which emphasized history as literature, history written with grace, style, and sympathetic imagination. Bury's fundamental allegiance belonged to the ideal of historical scholarship which had emerged in nineteenth-century Germany, and yet he felt the fascination of a Gibbon or of a Macaulay. Indeed, the example of Gibbon initially led him to assume that history written with superb literary style was inseparable from history animated by a *parti pris*. In the inaugural address, and especially in his response to Trevelyan's accusation that he wished history to be presented

⁵⁷ Bury's *History of the Papacy in the Nineteenth Century* (London, 1930), which is also intemperate, though less so than the *History of Freedom of Thought*, consists of posthumously published lectures.

⁵⁸ Heisenberg, "John Bagnell Bury," *Byzantinische Zeitschrift*, 27 (1927): 240.

⁵⁹ Introduction, *Selected Essays*, xxvii-xxviii.

⁶⁰ "The Use of Authorities" (unpub. lecture notes). I should like to thank G. R. Potter and Robert Schwoebel for allowing me to see these notes.

"without any literary dress," Bury attempted to steer a course between what he regarded as the demands of "the science of history" and those of traditional British historiography. For a Frederic William Maitland, with his literary genius and lack of self-consciousness about "the science of history," this presented no problem. Maitland's writings could have provided Bury with convincing evidence that lively and imaginative historical writing was compatible with impartiality. As it was, however, Bury was unable to resolve the conflicting claims of what might be termed the "British" and the "German" historiographical traditions.⁶¹ Inconsistencies and confusion resulted, mirroring the tension in Bury's own mind between these two differing modes of historical thought and practice. There remains the impression that, in dealing with the question of impartiality and of historical writing as a literary or artistic endeavor, Bury was not, so to speak, his own man. He could not break out of the terms of discourse set by an existing tradition and hence was not free to speculate *de novo*, as he did in considering the problems of historical explanation and of historical cognition.

THE KEYSTONE OF BURY'S CONCEPT OF "THE SCIENCE OF HISTORY" WAS THE IDEA of development, as he made quite clear in the inaugural lecture. Yet none of the subsequent discussion and criticism addressed itself to this topic. Even John Morley, a most perceptive and well-disposed critic, insisted that the more careful use of sources did not mean that a new "science of history" had been created.⁶² This is to miss Bury's point completely, since he had explicitly stated that it was the genetic approach, in conjunction with the critical method, that had changed the nature of history. Trevelyan, too, equated the scientific aspect of history with "the collection, collation, and valuation of historical evidence." He praised Bury's statement that history should be envisaged *sub specie perennitatis*, but related it to "the poetical sense of time and eternity."⁶³ In effect Trevelyan substituted his own essentially moral and aesthetic historical vision for Bury's emphasis upon the profound intellectual consequences of the genetic idea. The significance of viewing history as a process of genesis and development was, for Bury, that historical phenomena could thereby be perceived as coherent causal sequences amenable to scholarly investigation.⁶⁴ Insofar as the response to the inaugural address is any indication, the world of British historical scholarship either ignored or misunderstood Bury's position. As a result, his conception of "the science of history" was misconstrued and oversimplified. While this is the crucial point

⁶¹ This is not, of course, to say that German historical writing was devoid of either literary ability or bias: the difference is one of emphasis and self-image. Also, I am not referring to the Prussian school but rather to the Rankean tradition, which served as Bury's model of German historiography. A charming example of the way in which Maitland dealt with the question of the scholarly versus the literary elements in historical writing appears in his review of J. H. Round's *Commune of London*; see Maitland, *Selected Historical Essays*, chosen and introduced by Helen M. Cam (Boston, 1962), 259–60.

⁶² Morley, review of Frederic Harrison's *Theophrastus*, 576.

⁶³ Trevelyan, "The Latest View of History," 399, 412 n.

⁶⁴ "The Science of History," 6–11; also "Darwinism and History," 23–27.

for the purposes of this essay, there are other implications. For example, it is difficult not to detect in this incomprehension a measure of Bury's distance from his contemporaries among British historians. At the very least, it raises the question of whether his insistence that it was the genetic approach to the past that had created a new "science of history" was in fact unique within the British historical profession.⁶⁵

The idea of development provides the link between Bury's views on the nature of modern historical scholarship and his reflections about the purpose and direction of the historical process itself. Or, to express the matter somewhat differently, the idea of development is central to both the analytical and speculative components of his philosophy of history. In the former, understanding of the significance of change and development has crucial historiographical ramifications; in the latter, change and development are related to the concept of progress. Although occasionally Bury was guilty of an elision in which "development" and "progress" became synonymous, these lapses were few, and there can be no doubt about his awareness—and acceptance—of the distinction. Indeed, he neatly pointed out both the difference between and the relationship of the two ideas in 1909:

"Progress" involves a judgment of value, which is not involved in the conception of history as a genetic process. It is also an idea distinct from that of evolution. Nevertheless it is closely related to the ideas which revolutionized history at the beginning of the last century; it swam into men's ken simultaneously; and it helped effectively to establish the notion of history as a continuous process and to emphasize the significance of time.⁶⁶

To speak of progress, then, was to speak of a speculative theory about the meaning of the historical process, rather than to describe the process itself. As such, the idea of progress rested upon assumptions about human nature and the course of history which were not amenable to verification. What is more, the very notion of progress must be regarded as the product of a particular time and place: therefore it can have only relative value. To think otherwise would be to succumb to what Bury termed "the illusion of finality"—that is, the illusion that any doctrine represents final and absolute truth. In sum, the doctrine of progress "cannot be proved either true or false. Belief in it is an act of faith."⁶⁷

As a historian Bury emphasized that the idea of progress, irrespective of its validity, had increasingly become part of the European mentality since the seventeenth century. Yet his interest in progress did not stem solely from his awareness of its importance in modern Western history. He indicated his own sympathies by dedicating *The Idea of Progress* to its protagonists: to the Abbé de Saint-Pierre, the Marquis de Condorcet, Auguste Comte, Herbert Spen-

⁶⁵ The question of Bury's place within the world of British historical scholarship is one of the topics I am investigating as part of a larger study of the development of the British historical profession before 1914.

⁶⁶ "Darwinism and History," 27; also see page 25, where Bury states that the concept of progress implies a "teleological hypothesis" not involved in the genetic idea itself.

⁶⁷ *The Idea of Progress*, 1-7, 351-52. Also see the Introduction to Gibbon, *The Decline and Fall of the Roman Empire*, 1: xxix; "The Place of Modern History," 51-52; and *The Ancient Greek Historians*, 256.

cer, "and other optimists mentioned in this volume." The concept of progress was, for him, both an object of historical investigation and, as Baynes has commented, his "working faith."⁶⁸ The phrase is telling, expressing as it does the tentative and undogmatic nature of Bury's personal belief in the possibility of human progress. Although he admitted that intellectual advance must be clearly separated from the much more dubious question of ethical advance and that regression remained as likely a possibility as advance, Bury did believe that the progress of the West could be measured by the growth of rationality and of liberty.⁶⁹ For the future, the continued commitment to reason as the *modus operandi* in determining means and ends might gradually make, in Bury's words, "men's earthly home . . . a place fit for reasonable beings to live in."⁷⁰ This formulation, which speaks of the possibility of progress rather than of immanent and quasi-scientific "laws" of progress, bears little resemblance to the great speculative systems of the nineteenth century. Indeed, only by using the term loosely can one properly refer to belief in progress as Bury's speculative philosophy of history.

In any case, his "working faith" in progress is inseparable from his view of the purposes of historical research. For, despite his insistence upon the autonomy of history, he was convinced that the discipline served ends in addition to those of the advancement of learning. History, like all branches of knowledge, must be studied as if it had no ulterior aim; this precept, for Bury, was inherent in the assumption that history was a science.⁷¹ Yet that very understanding of history as a genetic process which was the source of its new scholarly status, when joined to faith in (or hope for) progress, meant that historical research took on new significance. In the course of comparing the Greek and modern conceptions of history, Bury expressed this idea explicitly: "For us, because we have a deeper insight into the causal connexion of past and future, because we have grasped the idea of development and dreamed the dream of progress, the reconstruction of history has become a necessity."⁷² By revealing to the present its own past, historical research played an effective role in clarifying choices for future action. Thus the historian works for posterity, knitting together past and present into a unity which he hopes will serve the future.⁷³ This mental outlook is what lies behind the oft-quoted passage of the inaugural address in which Bury spoke of history becoming "a more and more powerful force for stripping the bandages of error from the eyes of men, for shaping public opinion and advancing the cause of intellectual and political liberty. . . ."⁷⁴

⁶⁸ Baynes, *A Bibliography of the Works of J. B. Bury, with a Memoir*, 88.

⁶⁹ *The Ancient Greek Historians*, 256–57; *The Idea of Progress*, 1–7, 348–49. Bury's criteria of progress are similar to those of E. H. Carr (*What Is History?* chap. 6) and Morris Ginsberg (*The Idea of Progress: A Revaluation* [London, 1953], chap. 7), except that both mid-twentieth-century scholars insist upon equality as well as liberty as a touchstone of progress.

⁷⁰ *The Idea of Progress*, 349.

⁷¹ "The Science of History," 11–22, and *The Ancient Greek Historians*, 244–45.

⁷² *The Ancient Greek Historians*, 257–58.

⁷³ *Ibid.*, 253–58; "The Science of History," 16–19; "The Place of Modern History," 56; and *The Idea of Progress*, 347. The same emphasis upon posterity can be found in Carr, *What Is History?* 158.

⁷⁴ "The Science of History," 22.

This exalted view of the vocation of historical scholarship was shared by many historians in the nineteenth and early twentieth centuries. To conceive of their discipline as an instrument of enlightenment and progress not only conformed with a liberal intellectual and political stance but justified the prodigious expansion of historical research as well. Bury, who did not believe that progress was providential or immanent within the historical process, is a prototype of those historians who looked to the continued development and growing sophistication of "the science of history" as a means of human betterment. His interest in new modes of historical thought and research reflects both this attitude and his perception of himself as a professional historian keeping abreast of new developments within his discipline. He criticized the Rankean preoccupation with *Staatengeschichte* and called for the study of all aspects of the life of past societies.⁷⁵ And, like Lord Acton, his great predecessor as regius professor at Cambridge, he was convinced of the singular importance of modern history. He argued, again in contrast to the Rankean view, that recent history was a valid object of historical investigation.⁷⁶ In his desire to move beyond the traditional politically centered historiography and in his plea for the study of modern history, one senses an urgency, an insistence that the historian grapple with large questions and examine new approaches, so that "we may hope to charm from human history the secret of its rational movement, detect its logic, and win a glimpse of a fragment of the pattern on a carpet, of which probably much the greater part is still unwoven."⁷⁷ By extending the frontiers of historical knowledge, the historian was contributing not only to the advancement of learning but to the rational effort to understand the past as a means of shaping a better future.

Unfortunately, Bury's interest in broadening and deepening the content of historical scholarship remained by and large programmatic. His major works in Byzantine history are almost exclusively concerned with political and administrative history. He apparently felt that, at least in this field, much remained to be done in explicating what today would be called *histoire événementielle*, and that only later should economic and cultural history be undertaken. Another explanation of Bury's emphasis upon the political and administrative aspects of Byzantine history is that the continuity with Greek and Roman history drew him to Byzantine scholarship. This continuity was evident in politics and administration; hence these became the chief concerns of Bury's work.⁷⁸

In *The Ancient Greek Historians* and *The Idea of Progress*, however, Bury did succeed in breaking out of the mold of large-scale, political narrative. The former work is noteworthy—aside from its substantive value as sound in-

⁷⁵ *Ibid.*, 19–20, and "The Place of Modern History," 43–44.

⁷⁶ "The Place of Modern History," 51–59. Although Ranke did deal with contemporary history (e.g., in the later editions of the *History of the Popes*), he did not regard it as an independent field of historical study.

⁷⁷ "The Science of History," 59.

⁷⁸ R. H. Murray, Introduction to Bury, *History of the Papacy in the Nineteenth Century* (London, 1930), 1–11; Baynes, *A Bibliography of the Works of J. B. Bury, with a Memoir*, 6, 17; and Baynes, "Note," *Journal of Roman Studies*, 12 (1922): 207.

tellectual history—because of Bury’s willingness to move beyond delineation of the specific historiographical theory and practice of his protagonists to more general considerations about the nature of historical writing. His other scholarly foray into intellectual history, *The Idea of Progress*, reflects Bury’s assumption that ideas can be a powerful engine of social change. And yet the book itself was largely devoted to showing how the idea of progress originated and developed within the high culture of Western Europe over several centuries. Bury paid little attention to the social or political context of the idea or to how belief in that idea succeeded in becoming part of the collective European mentality. In the last chapters of the book Bury did attempt to shift his focus and explain how the idea of progress became popularized.⁷⁹ Without forcing these few passages to bear too great a burden, it can be suggested that they reveal glimpses of a broader approach to the study of social and cultural phenomena. Possibly the very exigencies of his subject had led Bury beyond his usual techniques of historical analysis. In the main, however, *The Idea of Progress* is intellectual history of the kind associated with A. O. Lovejoy, with its emphasis upon “tracing the morphology of a given concept over time.”⁸⁰ Neither this work, then, nor *The Ancient Greek Historians* provides evidence that Bury was able to translate his programmatic statements into innovative historiographical practice.

IT IS OBVIOUSLY INAPPROPRIATE TO PUT LABELS ON BURY’S HISTORICAL thought. To call him a “positivist” is to drain that term of substance; he neither shared the premises of the positivist world view nor wrote history in the manner of a Buckle or a Hippolyte Taine. Nor is Collingwood’s description of Bury as a “perplexed and inconsistent positivist” apposite. Bury was “perplexed and inconsistent” only when discussing history as literature and historical impartiality. Nor was it the influence of positivism which led to that confusion and inconsistency; it was rather Bury’s attempt to retain some fealty to the British tradition of historical writing which fostered perplexity. Perception of Bury as a kind of paradigmatic figure of historiographical “scientism,” sometimes conflated with his alleged positivism, has also been widespread. To accuse Bury of “scientism” could mean to charge that he misapprehended the proper relationship between history and literature, or suggest that he accepted the assumptions of the scientific school of history in the Rankean mode, or imply that he concurred with the positivist belief that laws can be derived from historical data. But Bury’s views cannot be comfortably subsumed under any of these rubrics. These persistent stereotypes have had the unfortunate effect of relegating the ideas of a major historian to the museum of historiographical antiquities.

⁷⁹ Chaps. 18 and 19.

⁸⁰ Quentin Skinner, “Meaning and Understanding in the History of Ideas,” *History and Theory*, 8 (1969): 48. See Skinner’s remarks on this approach, in which *The Idea of Progress* is cited, 10–12.

In the development of historical writing in Britain, Bury's primary importance lies in his endeavor, like that of his predecessor Lord Acton, to impress upon the British academic and intellectual community the changed nature and status of the discipline of history.⁸¹ The inaugural address proclaimed that henceforth historians must be considered (and consider themselves) professional scholars, responsible only to the canons of their craft. This implied not only adherence to the critical method, but awareness as well of the far-reaching implications of the genetic or developmental outlook, which constituted the essence of the nineteenth-century historical movement. By promoting the ideal of the professional, or "scientific," historian Bury helped to bring British historians into the mainstream of European historiography. Through his encouragement of research at Cambridge and his own historical writing, he provided a model of the professional scholar.⁸² He assailed what he termed the "insularity" of historical study at Cambridge with its emphasis upon English, at the expense of continental, history. Although unsuccessful in his efforts to include both more foreign history and some training in historical method in the Historical Tripos, his austere devotion to his calling and his cosmopolitan historical outlook did not pass unheeded.⁸³

Side by side with Bury the acolyte and proselytizer of the great tradition of continental historical scholarship, however, was Bury the "restless mind" that perceived unresolved problems within this tradition and sought to confront them. This makes him unique within the British historical profession in the years before 1914. For, although historians of the stature of F. W. Maitland, C. H. Firth, and A. F. Pollard were also professional scholars who encouraged rigorous research, they lacked Bury's self-consciousness about the historical craft itself. This self-consciousness led him to raise questions similar to those that agitated his continental and American counterparts: To what extent did the methods of the historian in fact provide "objective" knowledge? What was the nature of historical cognition? Did the uniqueness and individuality of historical phenomena mean that causal explanations were impossible? What of the relationship between history and the social sciences? How could the subject matter of history be broadened and deepened? Bury's efforts to speak to these questions afford additional evidence of the pervasiveness of what Georg Iggers has recently defined as the "crisis" of the scientific school of history.⁸⁴ But if historians in Europe and the United States felt the need to

⁸¹ Butterfield has noted that Acton was the "most self-conscious representative" of the nineteenth-century historical movement in England; *Man on His Past*, xv.

⁸² Bury helped to found both the Cambridge Historical Society and the *Cambridge Historical Journal*. See Temperley, Introduction, *Selected Essays*, xxxi.

⁸³ For Bury and the teaching of history at Cambridge, see Jean O. McLachlan, "The Origin and Early Development of the Cambridge Historical Tripos," *Cambridge Historical Journal*, 9 (1947): 96, 101-102. For Bury's influence at Cambridge, see Baynes, *A Bibliography of the Works of J. B. Bury, with a Memoir*, 50; Temperley, Introduction, *Selected Essays*, xxx-xxxii; G. P. Gooch, "The Cambridge Chair of Modern History," in *Studies in Modern History* (London, 1931), 319, and "Some Great English Historians," in *Historical Surveys and Portraits* (New York, 1966), 157-58; Murray, Introduction to Bury, *History of the Papacy in the Nineteenth Century*, xvi, lix; F. M. Powicke, *Modern Historians and the Study of History* (London, 1955), 131-32; and H. Temperley, *Research and Modern History* (London, 1930), 5.

⁸⁴ Iggers, *New Directions in European Historiography*, 18.

re-examine the nature and methods of their discipline, each undertook the task from within a specific national and social context. Perhaps the continuing strength in British intellectual life of the amateur tradition, which partially accounts for the late and incomplete professionalization of history in England,⁸⁵ chiefly shaped Bury's response. For Bury was in the awkward situation of at once endeavoring to foster the development of professional, "scientific" history and questioning some of the premises and formulations of the scientific school. To defend history as a *wissenschaftliche* activity and yet to point out the weaknesses and limitations of the Rankean historiographical tradition required an ingenuity bordering upon sleight of hand, given British attitudes and conventions. Bury's failure to move beyond occasional statements on methodological and epistemological matters may be at least partially attributed to fear that he might undermine the growing, but still incomplete, edifice of professional historical scholarship in England. Moreover, the lack of interest in questions of historiographical thought and practice among British historians must have engendered a sense of isolation.⁸⁶ With the exception of the inaugural address, none of Bury's speculative and theoretical essays were addressed to his colleagues within the British historical profession, and in his Cambridge lectures he made no attempt to communicate his awareness of, and interest in, theoretical problems. This remained the case even in the postwar years, when, to quote Carr, "the age of innocence" among British historians came to an end.⁸⁷ During this period there was a new interest in historical theory: Croce's books were translated and discussed and the work of Collingwood began to appear. Yet Bury, who continued to publish until his death in 1927, evinced no interest and made no contribution. Probably personal considerations played their part: Bury's health was failing, he was away from Britain much of the time, and he wished to complete his lifework in Byzantine history. In any event, although Bury was a significant figure in the professionalization of historical study in Britain, he had little specific influence in ending "the age of innocence" among his colleagues.

In the larger context of European historiographical thought, Bury's importance resides in his inclusion—and his alone within the ranks of British historians—among the protagonists of the "crisis" in early twentieth-century historiography.⁸⁸ Because this phenomenon has only recently engaged scholarly attention, interest has naturally focused upon the major figures, upon those whose efforts led to significant innovation or at least achieved wide notoriety. Hence, the program of Henri Berr and the *Revue de synthèse historique*,

⁸⁵ Felix Gilbert, "European and American Historiography," in J. Higham, L. Krieger, and F. Gilbert, *History* (Englewood Cliffs, N. J., 1965), 336.

⁸⁶ Suggestive of the naiveté with which British historians regarded historiographical questions in the years before 1914 is the analogy which one reviewer made in the pages of the *English Historical Review* between the work of Durkheim and the writings of the English social historian J. R. Green; "H. W. C. D." (probably H. W. C. Davis, later Regius Professor of History at Oxford), review of Henri Berr's *La Synthèse en Histoire*, in *English Historical Review*, 27 (1912): 181-82.

⁸⁷ Carr, *What Is History?* 21.

⁸⁸ Felix Gilbert has recently mentioned Bury in this connection; see Gilbert, ed., *The Historical Essays of Otto Hintze* (New York, 1975), 7.

the creation of the "New History" in the United States, and the *Methodenstreit* that grew out of Karl Lamprecht's advocacy of *Kulturgeschichte* have claimed attention and received investigation.⁸⁹ Yet reappraisal of the views of the less influential colleagues of these innovators is also essential if the full dimensions of the reorientation in modern historical thought are to be understood. From this point of view, Bury provides a case study of how far one historian was able to move in the direction of a reformulation of the tenets of traditional nineteenth-century historiographical theory.

Owing allegiance to no systematic philosophy and isolated from colleagues who shared his theoretical interests, J. B. Bury's conclusions were based solely on reading and reflection and on his experience as a working historian. His views regarding historical explanation and historical cognition were relatively sophisticated. They anticipated the concerns of modern analytical philosophy and the solutions accepted by many practicing historians. His opinions about the literary or artistic aspect of historical writing, however, cannot be said to have contributed to serious discussion about the extent to which history as a human study is by definition closer to art than to science; they merely echoed the adage that history should be written with flair and style, even at the cost of impartiality.⁹⁰ Although the bulk of his own historical work dealt with the distant past and with political history, he nevertheless called for the study of the recent past and of social, economic, and cultural history.

To locate Bury's thought within the framework of the "crisis" of early twentieth-century historiography is not to claim that he was a Berr or a Lamprecht; it is to suggest that what Meinecke termed the "silver age" of historical writing may have bred a more pervasive disquiet among historians than has hitherto been recognized. The example of Bury indicates that this disquiet is to be found within the ranks of those customarily described as traditional historians. Did other traditional historians share his sensitivity to unresolved problems and alternative approaches? One thinks for example of Seignobos, archetype of the much-maligned "*historiens historisants*," whose theoretical writings also show a groping toward new formulations. In short, perhaps many of the "fathers" (the traditionalists) were more complex than the "sons" (the innovators who displaced them) realized or could admit. But this is mere speculation, induced by appraisal of one historian who would have had little difficulty in accommodating himself to many twentieth-century innovations in historical scholarship.

⁸⁹ See especially John Higham, "The New History," in J. Higham, L. Krieger, and F. Gilbert, *History*, 104–16; William R. Keylor, *Academy and Community: The Foundation of the French Historical Profession* (Cambridge, Mass., 1975), chap. 8; Annie M. Popper, "Karl Gotthard Lamprecht," in Bernadotte E. Schmitt, ed., *Some Historians of Modern Europe* (Chicago, 1942), 217–39; Martin Siegal, "Henri Berr's *Revue de Synthèse Historique*," *History and Theory*, 9 (1970): 322–34; Robert Allen Skotheim, *American Intellectual Histories and Historians* (Princeton, 1966), chap. 2; and Karl J. Weintraub, *Visions of Culture* (Chicago, 1966), chap. 4.

⁹⁰ On this subject see Isaiah Berlin, "History and Theory: The Concept of Scientific History," *History and Theory*, 1 (1960): 24, in which the author reaches beyond the question of style to discussion of "a profounder sense in which the historian's activity is an artistic one."

Reviews of Books

GENERAL

MARINA WARNER. *Alone of All Her Sex: The Myth and the Cult of the Virgin Mary*. New York: Alfred A. Knopf. 1976. Pp. xxv, 400, xix. \$15.00.

GEOFFREY ASHE. *The Virgin*. London: Routledge and Kegan Paul. 1976. Pp. vi, 261. \$12.50.

Taking a structuralist approach, Marina Warner explores the history of the Marian cult through myth, art and literature, theological debate, Jungian psychoanalytical thought, and feminist theory. Her book is an adventure into the dynamics of a symbol, its roots in tradition, its cultural permutations in Christianity. The Virgin emerges as a social force of immense complexity which determines assumptions and attitudes just as she is determined by them. The symbolism of ancient fertility cults was assimilated into the various images of Mary, but underwent a process of spiritualization to accommodate the Christian doctrine of life hereafter. Mary's virginity, Immaculate Conception, and Assumption interlock with older versions of parthenogenesis but became dogma only after the Church found a way of identifying Mary with its concept of wholeness. Mary as the "purified" face of Eve embodies the stereotype of the eternal feminine in her subservient role to man. Though not new, the parallel is well documented and makes fascinating reading.

Warner is cautious, ambivalent. She states categorically that, as symbol of the ideal woman, Mary has been used as an instrument of oppression. Yet, as Christian muse she has inspired generations of men and women [*sic*] to create admirable works. This fragmentation of vision enables her to bypass the crucial role art plays in the development of the cultural behavior she deplores. While it is important to examine how the cult of the Virgin merged with the ideals of courtly love, a critical analysis of courtly love would have far greater significance, since it fostered the notion of love that dominates our society. Warner is careful to dissociate herself from what she calls "the extreme wing of the women's movement," whose theorists have drawn

some of her "findings" to their logical conclusions. Where her evidence leads to the existence of mother goddesses and matriarchs, she warns against taking matriarchy as historical fact. As for Mary, Warner believes that "the reality her myth describes is over." And she continues, ending with what could have been a more auspicious beginning, "although Mary cannot be a model for the New Woman, a goddess is better than no goddess at all."

Whether one agrees with Warner or not, her book is informative, superbly written, and amply illustrated. Not so *The Virgin*, by Geoffrey Ashe. One stumbles through a maze of ifs, buts, and howevers that upholds the family relationships Ashe has established in an attempt to reconstruct the historical Mary. "Whatever the truth about this prophecy, the family threads do begin to twitch," he writes, leaving one to ponder the validity of a portrait drawn from slim documentation and the twitchings of abundant speculation. Deluded but ambitious and cunning, Mary chose to live her life through the glory of her son, Ashe argues. She prepared Jesus' destiny by manipulating both the human and spiritual elements of her community. If the Annunciation is Mary's "delusion," the Immaculate Conception is a hoax made possible by the political genius of Rome. The Church endorsed the Virgin Birth to achieve religious unification and save itself, in the fourth century, from the impending disaster of heretical invasions. The strength and popularity of coexisting goddess-worship cults facilitated its assimilation.

Ashe relies on Graves and Jung: woman is an "authentic source of inspiration and life," and is again needed to save Christianity from collapse with her "intuitive wisdom." His fear that his book has helped feminism may be allayed. To re-evaluate the "mother-son" relationship ignores the daughter except as she is needed in the ministry to rescue a religion that oppresses her. The imbalance of power between the sexes is reinforced by postulating a "natural" polarization of stereotypes

(Wisdom/Logos), whose complementarity Ashe perceives as desirable.

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VERN L. BULLOUGH. *Sexual Variance in Society and History*. New York: John Wiley and Sons. 1976. Pp. xvi. 715. \$25.95.

The history of human sexuality has only just begun to liberate itself from the shackles of Victorian obscurantism and to come into its own as a respectable academic discipline, free of the preconceptions and fears of the past. In recent years a number of outstanding monographs have applied the canons of dispassionate research to the study of sex mores (e.g., Noonan and Flandrin on contraception, Monter on sodomy in the Suisse Romande, Le Roy Ladurie on the village of Montaillou, and Steakley on the German homosexual liberation movement), with the result that a more comprehensive study of prevalent sex mores and their opponents in the past is now possible. Vern L. Bullough's work is the first important product of this new historical effort which attempts a broad survey of sexual attitudes throughout the ages, placing special emphasis on sexual nonconformity, including transvestism, homosexuality, masturbation, and transsexuality, among other areas. It is especially suitable as an introductory textbook in the history of sexuality, summarizing as it does attitudes toward sexuality in Western and ancient civilizations, Islam, China, India, and Africa, with substantial bibliographical assistance for the interested reader. The intolerance and restrictive approach of Western civilization to matters sexual, enforced by the repressive mechanism of religion, is most glaringly highlighted in comparison to other, saner cultures.

Bullough's work suffers, however, from two limitations. Firstly, as a study of attitudes toward and theories about sexuality, it is difficult to gauge the relationship between theory and reality. The scholastic philosopher, for example, was frequently more concerned with defining the Christian ideal of sexual behavior, than with describing everyday mores. The apparent severity of the penitential code was often mitigated by resort to indulgences or dispensations. Furthermore, no effort is made to relate the acceptability of sexual variance to material and social realities, such as the rise and fall of the birth rate, or the availability of food. Instead, sexual behavior is too often seen as the result of a religious ideology, rather than of either psychogenetic or material transformations. Secondly, the admitted greater reliance on sources in English or in translation has meant that certain areas remain unexplored: the sexual views of the Taborites,

Frankists, and Anabaptists; the considerable extant record of persecution of sexual nonconformists by the Inquisition; the rather sophisticated but as yet untranslated thirteenth-century manuals of penance associated with the Paris schools; and records of the mass execution of Dutch sodomites carried out in 1730/31. Also, the conditions which could lead to the paradoxically simultaneous rise of libertinism and Puritanism, or Gnosticism and Priscillianism, remain unexplored.

Some other tempting subjects are alluded to, but not pursued, e.g., the records of the Florentine *Ufficiale di notte* empowered to seek out and punish sex offenders; or Peter Damiani's unique pamphlet *Liber Gommothianus*, directed against clerical sodomites, which is summarized all too briefly. Other areas, such as eighteenth- and nineteenth-century medical views of sexuality, or the Indian sex manuals and doctrines of Tantrism are given the attention they deserve; and where historians differ, or texts are difficult to interpret, Bullough is quick to point out the difficulties. In short, this work provides a good introduction to the history of sexuality, and provides guidelines for future study in an area which has thus far been only superficially treated.

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JEREMY L. TOBEY. *The History of Ideas: A Bibliographical Introduction*. Volume 2, *Medieval and Early Modern Europe*. Santa Barbara, Calif.: Clio Press. 1977. Pp. xii, 320. \$19.75.

From the gleam in Arthur O. Lovejoy's mind's eye some two generations ago the "history of ideas" has grown from a small club to a full-fledged discipline; and this reference work testifies to its success and status, if not to its popularity among historians *tout court*. This bibliographical survey, containing some 1,500 authors and well over 2,000 titles, extends roughly from the ninth to the seventeenth century and is organized according to the following rubrics: 1.) general introduction, 2.) philosophy, including political and social thought and historiography, 3.) science, physical and biological, and technology, 4.) religion, and 5.) esthetics, including visual and musical arts and several national literatures. Each category is arranged chronologically and includes sections on some prominent figures. It is informative, generally abreast of current scholarly issues, and offers numerous critical discussions, usually balanced and mild and almost always orthodox.

Here there is space only for a few words about its limitations. It is admittedly elitist as well as rationalistic, "the study of ideas used by educated people

to comprehend their universe and to rationalize their social institutions, arts and religion." So psychohistory is generally frowned on (because of its lack of demonstrability), and what we have learned to call the "social history of ideas" is skirted entirely. Nor does the problematic posed for intellectual history by printing (not to mention the name of Elizabeth Eisenstein) come in for discussion. At the same time the book offers what seems to me a rather eccentric selection of the classics—Henry Osborn Taylor but not Egon Friedell, Richard Foster Jones but not J. B. Bury, John Addington Symonds but not Georg Voigt, Hiram Hayden but not Herschel Baker. Admittedly but unaccountably, it deliberately exclude: all Italian works and is highly selective in its choice of German and French (no mention, for example, of Bataillon, Dempf, Renucci, Schramm). Even in English there are surprising omissions (Lewis Beck, Marcia Colish, Arthur Ferguson, J. H.exter, Hans Kohn, Francis Oakley, who all span medieval and modern). Perhaps more serious is the neglect of vital works in such technically marginal fields as linguistics, law, anthropology, and social and economic thought. Not lack of diligence but the crudeness of his bibliographical net leads Tobey to overlook Schumpeter's *History of Economic Analysis*, for example, while duly recommending Isaac Asimov's *Short History of Chemistry*.

Nevertheless, historians will find this work to be a serviceable, and will probably judge it to be a standard, guide to a large interdisciplinary territory, many parts of which are still uncharted. If it has limitations for explorers on the frontiers, it offers an admirable survey of the cultivated areas, especially for undergraduate and graduate reading and teaching.

DONALD R. KELLEY
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RUSSELL MCCORMMACH, editor. *Historical Studies in the Physical Sciences*. Volume 7. Princeton: Princeton University Press. 1976. Pp xxxv, 489. \$28.50.

Previous volumes of *Historical Studies in the Physical Sciences* have led readers to expect useful contributions of high quality, with the majority of papers emphasizing the history of physics in the twentieth century. Occasionally there is a nod to other fields and to other centuries. The present volume will not be disappointing, for the papers continue in the same mode; doubtless the subjects and eras reflect roughly the areas where scholarly activity is presently concentrated.

Three of the nine papers in this volume deal with Einstein's theories, as does Russell McCormmach's "Editor's Foreword." Tetu Hirosige's posthumously published study on the true novelty

of Einstein's theory of relativity brilliantly shows that the scientific community had to be liberated completely from its mechanistic world view before the theory won acceptance. Hirosige accomplished his demonstration by evaluating Mach's influence on Einstein and by re-examining that troublesome nineteenth-century entity, the ether, and assessing the role played by Poincaré and Lorentz in attempting to accommodate the ether to the real world. Stanley Goldberg's contribution analyzes Max Planck's response to Einstein's theory of special relativity.

Joan Bromberg's short paper discusses the transformation by quantum mechanics of notions about annihilation (and creation) of fundamental particles and thus introduces those papers not specifically connected with Einstein. Henry Guerlac's examination of the nature and extent of Lavoisier's collaboration with Laplace in one sense parallels Lewis Pyenson's study of Einstein's relationships with his collaborators, Laub, Ritz, and Freundlich. Both papers anatomize one of the more delicately balanced mechanisms encountered in studying scientific creativity. Regrettably, McCormmach in his "foreword" did not draw attention to the interesting parallels the two papers invite. P. M. Heimann removes some of the formidable thicket of myth grown up around the work of Robert Mayer by looking to see how Mayer's concept of force functioned within his broad-based natural philosophy.

Most interesting and most instructive of all the essays in this collection—and, incidentally, the longest—is R. G. A. Dolby's "Debates over the Theory of Solution." Historical change is Dolby's real subject. Promulgation of Arrhenius's theory of electrolytic dissociation and J. van't Hoff's solution theories precipitated intense, relatively long-enduring discussion. While the details of the discussion as narrated by Dolby are in themselves interesting, Dolby's consideration of how the debate process can modify the content and experimental goals of science lifts the paper out of the category "internalist history" and into one with far wider implications.

Romualda Sviedrys' "The Rise of Physics in Britain" likewise shifts from purely internal concerns, since the author's topics are the institutionalization of academic physics in Britain, the relationships between physical research problems and industrial research problems, and industrial research problems and the changing patterns of science education. G. K. Roberts' study of the Royal College of Chemistry is a companion piece to Sviedrys' study. Inclusion of these three last essays bodes well for the profession and for McCormmach's journal. Study of factors like institutionalization, professionalization, and politi-

cization of science and scientists will certainly improve our understanding of the history of science.

JUNE Z. FULLMER
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ALLAN CHASE. *The Legacy of Malthus: The Social Costs of the New Scientific Racism*. New York: Alfred A. Knopf. 1977. Pp. xxvii, 686, xviii. \$17.95.

Commencing in early nineteenth-century England and lasting to the present day, scientific racism has substantially influenced British and American public policy. Couched in pseudoscientific terms, it has fostered the myth that civilization is divisible into two distinct races—a healthy, wealthy, educable, and usually white elite, and the sick, poor, uneducable, and often nonwhite masses. Fostered by Thomas Malthus and amplified by late nineteenth-century Social Darwinists, scientific racism justified high death rates among the poor as a sign of progress: inferior folk were dying off, the superior were surviving, and the quality of the population was therefore improving. But late Victorian public health movements in Britain and America reduced lower-class mortality rates and consequently perpetuated the “unfit.” For this reason, scientific racists like Charles Davenport and Harry Laughlin became active in eugenics crusades for compulsory sterilization of the “genetically inadequate.” Until World War II, their “achievements” were substantial: state laws authorizing sterilization of “mental defectives,” a 1924 federal immigration statute with ethnically discriminatory quotas, suppression of the discovery that pellagra was a noninheritable diet deficiency disease, and advisory functions in the formulation of Hitler’s mandatory sterilization program. World War II marked the nadir for Anglo-American scientific racists. It was difficult for them to muster support for Nazi scientific dogma in Western democracies. But in the postwar decades, scientific racism returned to respectability. “Population bomb” theorists like Paul Ehrlich, Arthur Jensen, and William Shockley gained a public following by crusading for sterilization and birth limitation as the proper remedy for mass poverty. The legacy of Malthus had continued.

These are the broad lines of Allan Chase’s thoroughly researched narrative. Chase ties this narrative to an important thesis—that Anglo-American scientific racists have always been only superficially concerned with race, religion, or nationality. More fundamentally, they have sought to perpetuate upper-class rule by the world’s affluent minority and to keep down the impoverished majority. The racist element in their doctrine has only been a manifestation of their deeper desire for

upper-class hegemony. Unfortunately, Chase does not prove his important assertion, because he provides neither systematic nor detailed social, psychological, and ideological analysis of his specific cast of scientific racists and their supporters. He would have had difficulty constructing a convincing proof. After all, it is no small feat to demonstrate that figures like Oliver Wendell Holmes, Jr. and AFL president William Green were ideological bedfellows in common pursuit of upper-class hegemony. Chase’s class analysis is further complicated by those intellectuals who joined the scientific racist crusade because it was a promising route to federal grants and academic recognition. As Robert Nisbet has demonstrated, Anglo-American intellectuals have all too often concocted “objective” tests and “scientific” proofs for absurd propositions solely to enhance their stature in the academy or with the research foundations.

This is all to say that Chase provides a solid narrative history of scientific racism and a potentially valid class analysis to explain its existence. If he had only developed his class analysis to account for the seemingly uncommon bedfellows who joined the pseudoscientific crusade, *The Legacy of Malthus* might have been one of the major books of our time.

LAWRENCE J. FRIEDMAN
Bowling Green University

KOSTAS AXELOS. *Alienation, Praxis, and Technē in the Thought of Karl Marx*. Translated by RONALD BRUZINA. Austin: University of Texas Press. 1976. Pp. xxxiii, 401. \$12.50.

This is the first book by Kostas Axelos to appear in English translation. Though relatively unknown outside France, Axelos is a highly regarded Marxist thinker in his own country. As a member of the *Arguments* group and a philosopher rooted in phenomenology and Heideggerian existentialism, he has had considerable impact both in illuminating the content of Marx’s thought and in attempting to think beyond it.

The book under review is an explication of Marx’s central ideas concerning alienation, *praxis*, and technology. By focusing particularly on the works of the early Marx, Axelos goes over what may appear familiar territory, yet he does so with enough insight that one is able to see several aspects of Marx in a new light. The book is arranged by topical chapters, each building on the preceding one in such a way that the logic of Marx’s thought seems to unfold naturally. By the end of the book the reader is able to see clearly relationships and interconnections which were only suggested at the beginning. Providing a sound discussion of Marx’s views on knowledge and *praxis*,

labor and technology, civil society and the state, Axelos also deals with some of the more neglected areas of Marx's work: namely, his treatment of the family, the relationship between the sexes, and the place in religion, art, poetry, and science in modern society. Except for the "Conclusion" (in which Axelos asserts his own views more forcefully) these are perhaps the best chapters in the book. There is also a worthwhile treatment of Marx's notion of the need to "abolish philosophy" by realizing it (chapter 14), and of his conception of "communism as fully developed humanism" (chapter 15). On the latter topic Axelos spends some time delineating the shape of a future communist society as Marx understood it, while at the same time criticizing it on theoretical and practical grounds.

This leads to the second theme of the book, the critique of Marx from within the framework of Marxism itself. In this respect Axelos raises several questions concerning what he thinks are flaws or lacunae in Marx's world-view. Most notably Axelos finds that Marx is still fundamentally metaphysical, having only completed, not overcome, the Western philosophical tradition; that he is still locked into dualistic modes of thought on key issues; that his notion of subjectivity is inadequate; and that he is overly optimistic about the ability of technology to eliminate the conditions of alienation and thereby prepare man for communist society. On this last point, Axelos sees technology as much more problematical than Marx did. In his concluding chapter subtitled "Open Questions," he suggests that perhaps the advancement of technology, rather than laying the groundwork for a future communism, may actually perpetuate the division of labor *ad infinitum*, thereby making an ideal society forever impossible.

On the whole, this book can be recommended as a good introduction to both the virtues and shortcomings of Marx's thought. The translation by Bruzina is an adequate job on a difficult text, though his introductory comments on Axelos are not as clear as they could be.

DAVID GROSS
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SPIRO KOSTOF, editor. *The Architect: Chapters in the History of the Profession*. New York: Oxford University Press. 1976. Pp. x, 371. \$19.50.

Rarely does the opportunity arise to provide an entirely new insight into a major profession. Concerning architecture, this opportunity has existed since long before the founding of the Society of Architectural Historians in 1940. Since then the SAH has devoted great collective effort to the history of buildings, but relatively little attention to the profession that produced those buildings. Not

since M. S. Briggs published *The Architect in History* in 1927 has anyone surveyed the practice of the building art through the centuries. Now a recent past president of the SAH has organized and edited a collaborative effort to present "chapters in the history of the profession" (principally in the West) to a general reading audience. The resultant product goes a long way toward filling the gap and shows none of the weaknesses one might expect from a collection of essays by different authors.

Architecture as a profession—at least in the United States—has been in a state of crisis since the 1960s. The cherished conceit of architecture as the "Mother of the Arts" stands in crass conflict with the reality of architecture as the hired hand of big capital. "Architects in modern America indeed came to terms with the facts of industry and commerce, but typically at the cost of their ethical responsibilities as independent professionals. The ethics of the individual architect were replaced by the ethics of the architectural office, and the more the architectural office resembled businesses in general, the more did its ethics resemble those of the business world."

Moreover, only a fraction of what gets built in the USA is designed by architects (as historically has been the case everywhere), and only a fraction of the practitioners are members of the American Institute of Architects. It is thus no wonder that the struggle to find a professional identity continues. This book elucidates the nature of architects' varying roles through the centuries and thus explains the (recurring) identity crisis in the profession.

The University of California at Berkeley has been an epicenter for this crisis; the chairman of the Department of Architecture there was appointed head of a "Committee for the Study of the Future of the Profession" by the AIA. Appropriately, this book is also a product of Berkeley. As architectural historian there, Kostof in 1974 initiated a new course—presumably the first in the country—on the history of the architectural profession. Many of the contributors to this volume collaborated on that course. Kostof's own contributions include the chapters on "The Practice of Architecture in the Ancient World: Egypt and Greece" and "The Architect in the Middle Ages, East and West." Among the other authors, William MacDonald contributes a chapter on "Roman Architects," Leopold Ettlinger discusses "The Emergence of the Italian Architect during the Fifteenth Century," while Catherine Wilkinson writes on "The New Professionalism in the Renaissance." "The Royal Building Administration in France from Charles V to Louis XIV" is the topic of Myra Nan Rosenfeld, research curator at the Montreal Museum of Fine Arts.

John Wilton-Ely from Britain writes on "The

Rise of the Professional Architect in England.” Joan Draper discusses “The École des Beaux-Arts and the Architectural Profession in the United States: The Case of John Galen Howard.” “Architectural Education in the Thirties and Seventies: A Personal View” is the contribution of Joseph Eshrick, and Gwendolyn Wright’s subject is “On the Fringe of the Profession: Women in American Architecture.” Finally, Bernard Michael Boyle contributes the concluding chapter summarizing “Architectural Practice in America, 1865–1965: Ideal and Reality.”

The book benefits greatly from the fact that each chapter circulated to the other authors for their comments and suggestions. The material is impeccably researched, objectively interpreted, and interestingly presented. The writing is of consistently high quality, the editing well done. The book reads as a continuous narrative. Eliminating footnotes in favor of bibliographical notes appended to each chapter makes for better reading plus enhanced access to primary and secondary sources.

The Architect is one of those infrequent books that satisfies very well the disparate needs of the general reader, the student, and the serious scholar. It is a significant contribution to our understanding of history.

RONALD WIEDENHOEFT
University of Utah

ERNEST S. DODGE. *Islands and Empires: Western Impact on the Pacific and East Asia*. (Europe and the World in the Age of Expansion, volume 7.) Minneapolis: University of Minnesota Press. 1976. Pp. xvi, 364. \$16.00.

This book is a survey of Western expansion into the Pacific islands, including Australia and New Zealand, and into East Asia, down to about 1900. Ernest S. Dodge is at his best in describing first contacts and early trade and social relations in the islands. His decision to include the beginnings of English settlement in Australia and New Zealand was a sound and interesting one, leading to clear perceptions of the interrelations of these developments with the islands of the South Pacific. But the promise shown in some sections is undercut by major weaknesses in others and in the work as a whole.

First, the book has little unity of scope or theme. The juxtaposition in one volume of Western expansion into the South Pacific and into East Asia raises the interesting prospect of a history of the opening of the Pacific, keeping in view the growing Western involvement on all its shores and in all its waters. But almost nothing is done with Russian expansion into the North Pacific, the English and other raids into the “Spanish Lake,” the late-

eighteenth-century Hispano-Anglo-Russian rivalry on the west coast of North America, or the “gold rush” movements of population by sea to both California and Australia. The geographical-scientific element in the voyages of exploration receives no sustained treatment. The history of the Philippines has been largely ignored. Although he need not have given balanced treatment to all these themes, when they are scarcely mentioned we cannot see the whole Pacific context of the developments that Dodge wants to emphasize. Themes that could have linked the Pacific and East Asia sections of the book, but are treated unsystematically in both, include the role in the expansion of the Canton trade in the early nineteenth century of Pacific sandalwood, furs, *bêche-de-mer*, and so on; the roles of East Asian immigrants in settling the islands and other shores of the Pacific; and the comparisons and contrasts in the activities of Western missionaries, merchants, advisors, and adventurers in the Pacific and East Asia.

Second, even within the areas he sets out to discuss, Dodge’s coverage is erratic. He seems to lose interest in the Pacific islands after the drama and turmoil of early encounters and has little on Hawaii or Tahiti after 1840 or Fiji after 1874. He does much more with English and American expansion than with French, with Protestant missions than with Roman Catholic. Descriptions of the dramatic changes in indigenous politics in Hawaii, Fiji, and Tahiti and of the astonishing response to the missionaries in many Pacific islands fail to place these developments in contexts of native political and cultural patterns. The discussion of Chinese and Japanese responses to Western challenges in the nineteenth century shows no familiarity with the changing interpretations of Ch’ing China and Tokugawa Japan that have so profoundly altered our views of their nineteenth-century interactions with the West.

Third, Dodge has not paid sufficient attention to problems of narrative coherence. There are entirely too many confusing shifts backward and forward in time. The failures to give coherent accounts of the careers of Kamehameha I in Hawaii and Ponape I in Tahiti are partly the results of a topical organization of the South Pacific chapters. Clearly some such organization was necessary in these sections, but it could have been more effectively reconciled with the need for clear narrative sequences. The description of the Japanese “closing of the country” is very hard to follow.

Fourth, the book is marred by factual errors, especially in the East Asia sections, and by inadequate bibliography. Thus we have European responses to the South Seas without Diderot, Melville, or R. L. Stevenson; the Fiji sandalwood trade without Lockerby’s journal; Christian missions in

China without Paul Cohen; and even the Canton trade without Dermigny! There is not a single work in the bibliography in any language except English.

Groundwork has been laid by many scholars for someone to write the history of the opening of the Pacific. This book will offer that person some guidance and stimulation, and a partial sense of the challenging intricacy of his subject.

JOHN E. WILLS, JR.

University of Southern California

ANCIENT

R. J. HOPPER. *The Early Greeks*. New York: Barnes and Noble. 1977. Pp. 257. \$25.00.

All historians of early Greece must confront the central problem of the inconclusive nature of the evidence. R. J. Hopper, therefore, faces a difficult task in attempting to present a general history of Greece from the early beginnings in the Bronze Age to the "clear dividing line in the mid-sixth century" which will be of use to "a wider public outside the various groups of specialists." Unhappily, he has succeeded neither in providing a systematic historical analysis nor in satisfying the requirements of his intended audience.

The book is logically organized and contains few factual errors. Yet, from the first pages, the reader encounters the vagueness which is the study's major failing. The declared limits of the study present an immediate and serious problem in this regard for the author has not determined in his own mind when Greek history truly begins; that is, when can we date the arrival of the first proto-Greek speakers? While the difficulty is raised as early as page 11 of the introduction, Hopper still wonders in his chapter dealing with the Dark Age whether "'Greek' is rightly used here and earlier." There is more assurance about the value of the terminal point of the work although the author finds it necessary to carry his account well into the Classical period.

One must admire the quantity of detailed data gathered by Hopper. The work is based upon a wide variety of archeological, literary, mythological, and geographical evidence although bibliographic references are minimal, presumably to suit the needs of a general audience. The extraordinary amount of detail is, however, never integrated into a coherent, compelling account, and herein lies the failure to speak to the intended audience. Confronted with the data, the reader is left wondering about the resolution of the "nice problem" (p. 29), "the most intractable problem relating to early Sparta" (p. 174), and the "puzzle of the first or-

der" (p. 198). While firm solutions are impossible in many cases, any reader, but especially a non-specialist, hopes to have the evidence marshalled and weighed, if only according to currently defined probability. Hopper is reluctant to offer conclusions. Moreover, on those rare occasions when he does commit himself to a position, his evidence is either not presented (pp. 39, 64) or is not persuasive (pp. 47, 133, 179ff.). Particularly disconcerting is his trust in mythological data.

The need does exist for an up-to-date survey of early Greek history. For the moment, however, the reader should still turn for a coherent, readable account to the surveys of C. G. Starr and M. I. Finley, or to the more specialized studies of the Bronze Age by Chadwick and Hooker, of the Dark Age by Desborough and Snodgrass, and of the Archaic period by Jeffery.

C. G. THOMAS

University of Washington

COLIN M. KRAAY. *Archaic and Classical Greek Coins*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xxvi, 390. \$60.00.

Until the last few years, Charles Seltman's *Greek Coins* (2d ed., 1955) has been the standard account in English of the historical development of Greek coinage (while B. V. Head's *Historia Numorum* [2d ed., 1911] is still the basic encyclopedic treatment of the material). Seltman's work, speculative on many points, was, however, becoming increasingly outdated. In 1966 Max Hirmer's volume of splendid photographs with extensive German commentary by P. R. Franke was published in English with a new text by C. M. Kraay. Subsequently, there appeared G. K. Jenkins' *Ancient Greek Coins* (1972). This is a less detailed work than Kraay's new book, but it has a wider scope, including Greek coinage of Alexander the Great's empire and after, and it is illustrated with outstanding photographs, many in color. A sequel to Kraay's volume on Hellenistic coinages is in preparation by Otto Mørkholm.

For Greek coinage down to Alexander, Colin M. Kraay's new book now replaces Seltman definitively. It is the work of an eminent numismatist who has not been content to present what is known of the various Greek series, some of which have been studied in detail, others partially, some not at all, but has considered the problems of each mint in turn and has collected and examined the evidence as far as possible without attempting exhaustive mint studies. The result is that Kraay is able, for the first time, to present a view of the relative scale of production of the Greek coinages and in so doing moves Greek numismatics toward the economic history of Greek money.

The problems of the origin of coinage and the history of its first century are subjects in which the progress between Seltman and Kraay is most apparent. As a result of twenty years research (to which Kraay has made major contributions) it is clear that coinage came into being at the end of the seventh century B.C. with electrum striking in Asia Minor and that the principal silver coinages in Greece did not begin until the second quarter of the sixth century. (Still lower dates for these events have now been proposed by M. Price and N. Waggoner, *Archaic Greek Coinage, the "Asyut" Hoard* [1976].) The purposes served by the first coinages have long been assumed to be consciously mercantile. In addition to the intrinsic value of early electrum, and even silver, coins, which were too valuable to have been useful in retail transactions, Kraay's own work (especially "Hoards, Small Change, and the Origin of Currency," *Journal of Hellenic Studies*, 84 [1964]: 76) has shown the extent to which coins circulated in delimited provinces. The translation of bullion into coin therefore effectively limited rather than extended its circulation, thus protecting local supplies by the overvaluation produced by the cost of minting, if not by deliberate added value assigned to the issues. Electrum, which varied greatly in intrinsic worth depending on its relative content of gold and silver, could only be made a uniform medium of exchange by the assigned value of the coin.

Coinage is now seen to have been rapidly taken up throughout the Greek world. The first issues in South Italy, for instance, are no longer to be considered retarded imitations of coinage in Greece and Asia Minor but are just as old as the Aegean coins. The stimulus for the expansion of minting after about 500 in northern Greece appears to Kraay to be the result of Persian taxation, rather than bullion export as previously believed.

Greek coinage of the fifth century has long had a fixed point in the "Damareteion," a ten drachm piece struck, according to tradition, at Syracuse around 480 to honor the wife of the tyrant Gelon. The "Damareteion," has long been identified with an existing Syracusan silver coin of that weight. In recent years, however, Kraay has led an attack on the "Damareteion," proposing to date the existing coin to the 460s. The resulting shifts in the Syracusan series are of great significance. Due to the discovery of an otherwise unknown coin of Aitna (the city of Katane, renamed in 476) which was struck from dies possibly prepared at Syracuse and closely related to the "Damareteion," it is clear that a displacement of the traditional date is inevitable, though possibly less drastic than that of Kraay's solution.

In Kraay's book, the historian will find the best review of the effects of the Athenian coinage de-

cree, for which the date 448 has been further supported by B. D. Meritt, "Perikles, the Athenian Mint, and the Hephaisteion" (*Proceedings of the American Philosophical Society*, 119 [1975]: 267), in relation to the authorization of the building of the Temple of Hephaistos in the Athenian Agora. The most evident advance in Kraay's treatment of the fourth century is his appraisal of the coinages with Corinthian types struck at cities of the Adriatic and in Sicily in relation to the economic stimulation afforded by Greek recolonization of Sicily under Timoleon.

In design and printing the book is a pleasure to read. The plates were made from casts and resemble those of the British Museum's *A Guide to the Principal Coins of the Greeks*. They illustrate 1,110 specimens.

R. ROSS HOLLOWAY
Brown University

RAPHAEL SEALEY. *A History of the Greek City States, ca. 700-338 B.C.* Berkeley and Los Angeles: University of California Press, 1977. Pp. xxi, 516. Cloth \$15.00, paper \$7.85.

In a series of provocative essays Raphael Sealey has challenged many widely held assumptions about the nature of Greek political life. Highly skeptical of explanations depending on ideology, class tensions, and political "parties," and distrustful of social and economic generalizations, he has focused attention on regional and personal conflicts and animosities. The present work carries on his challenge to conventional wisdom; one is eager to see what results emerge when his principles are consistently applied in an extensive historical narrative.

The title promises too much and too little. The book excludes the city states of Magna Graecia and of other important areas. At the same time it attempts to offer more than a dogmatic narrative: "it discusses the problems which I consider important and the hypotheses which I find illuminating." Within five hundred pages it is, of course, impossible to treat very many problems in this way. Thus the text often seems to pause excessively on one problem only to hasten over a series of equally crucial issues. The topography of the Hollows of Euboea receives more attention than the Sophistic Movement; Phidias and Socrates are not found in the index; frequently the narrative becomes a Xenophontic quest for wars and battles with no time for social, artistic, or intellectual developments.

Neither 700 nor 338 provides a satisfactory terminus. The origin of the developments with which the work is concerned is often closer to the mid-eighth century, and the Battle of Chaeronea in 338

B.C. is surely a misleading point at which to stop a history of the Greek city states. Sealey is aware of the difficulty and construes the battle not as "that dishonest victory fatal to liberty" but as a "severe, though temporary check to the federal movement among the Greeks." But without a fuller treatment of Hellenistic federalism the statement is of little help. The treatment of earlier Greek leagues is also inadequate. Many readers, for example, may be misled by the titles of the last two parts of the book ("The Era of Hegemonic Leagues," i.e. the period 479–386 and "Leagues of More Equal Type," i.e. the period 386–338). Indeed perhaps it is not only the reader who is misled; Sealey gives an uncharacteristically rosy picture of the Second Athenian Naval Confederacy and is thereby left without an adequate causation for the Social War.

Despite these criticisms, the treatment of individual problems is often incisive. Sealey's strength is in the presentation of conflicting evidence and theories and in encouraging the reader to make up his own mind. He shows good reason to doubt the authenticity of the Great Rhetra and gives an excellent exposition of some widely held theories about Greek tyranny. But the treatment of Solon is so agnostic that it is hard to see why there was any fuss about him in antiquity. Often implicit is a denial of the significance of changes in the political institutions of Athens. The Pisistratid tyranny emerges as a coalition of powerful aristocrats representing "the eastern Attic cause." Cleisthenes' reforms substituted the dominance of city families. "The choice for Athens was not between tyranny and liberation; indeed there was very little choice." There are many difficulties in this analysis, and the problems become more intense as Sealey treats the question of how Athenian politicians gained their support. The assumption is of facile manipulation: "A man who could provide the crew of a trireme . . . could likewise bring them to vote in the public assembly." Herein the basic difficulty of Sealey's approach. Roman historians can rely on *amicitia* and *clientelae* to explain political support. Greek historians must still keep searching for an adequate etiology for political groupings and power. As they search they would do well, perhaps, to bear in mind Pericles' advice to his fellow commanders always to remember that they were dealing with free men, with Greeks, with Athenians.

W. R. CONNOR
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MOGENS HERMAN HANSEN. *The Sovereignty of the People's Court in Athens in the Fourth Century B.C. and the Public Action against Unconstitutional Proposals*. Translated by JØRGEN RAPHAELSEN and SONJA HOLBØLL. (Odense University Classical Studies,

volume 4.) Odense: Odense University Press. 1974. Pp. 80. D kr. 50.

MOGENS HERMAN HANSEN. *Apagoge, Endeixis and Ephegesis against Kakourgoi, Atimoi and Pheugontes: A Study in the Athenian Administration of Justice in the Fourth Century B.C.* (Odense University Classical Studies, volume 8.) Odense: Odense University Press. 1976. Pp. 171. D kr. 60.

In the first of these monographs Mogens Herman Hansen's aim is to discover whether ultimate sovereignty in the Athenian constitution of the 4th century B.C. resided in the Assembly or in the public courts. He chooses as a test case the one procedure in which the powers of *ekklesia* and *dikasterion* most often overlapped: the public action against unconstitutional proposals by which laws or decrees either proposed in or enacted by the Assembly or the *Nomothetai* could be blocked by indicting their proposer(s) for unconstitutional measures. The trial was held in one of the public courts.

Hansen presents a catalogue of all known examples of this public action (39 *graphai paranomon* and 6 indictments of an inappropriate law) and argues from these specific instances "that any decision made by the Assembly may ultimately be reversed by the courts whereas no decision made by the courts can ultimately be reversed by the Assembly." Therefore, the sovereignty of the People's Court was ultimate whereas that of the Assembly was immediate and potentially subordinate.

Hansen offers on this point a welcome corrective to the widely held view that it was ultimately in the *ekklesia* that the people of democratic Athens expressed their true sovereignty. He is also careful not to press his conclusions too far, for he recognizes that even though jurors in a given panel were under oath, over thirty years of age, and considerably fewer in number than members of the Assembly, who were unrestricted by an oath and had only an age limit of over twenty, there was such enormous overlap in personnel between the two that to speak of different political ideologies in the two bodies is to talk nonsense.

In *Apagoge, Endeixis and Ephegesis*, Hansen examines three closely related legal procedures in classical Athens for the prosecution of thieves caught in the act or caught with stolen goods, exiles returning without reprieve to enter and defile forbidden places like the Agora or sanctuaries, state debtors and other disenfranchised Athenians who attempted illegally to exercise the rights of citizenship. As late as the 4th century B.C. self-help could still be invoked against criminals in the first two categories to the point that they could be killed on the spot. Under *apagoge* criminals could be haled before the appropriate magistrate and arrested by

the prosecutor. Immediate interrogation ensued, and a confession was followed by execution without trial. If the charge was disputed, a trial was held under the presidency of the appropriate magistrate. In *endeixis* arrest was not mandatory, but a written denunciation had to be filed with the competent magistrate prior to trial. In *ephegesis* the prosecutor denounced the alleged criminal to the magistrates and entrusted incarceration until the time of the trial to them.

The intricacies of these procedures are examined in six chapters, of which chapter three, on disenfranchisement, is the longest and most difficult to read and chapter five the most controversial. In the latter, Hansen argues that "the administration of justice was almost unchanged from Drakon to Demosthenes and . . . the reaction against crimes in Athens must be described as 'archaic' even in the fourth century. . . . It is impossible to demonstrate any important development of the Athenians' reaction against criminals guilty of homicide or offences against property." Many scholars have postulated a development from archaic self-help to the more restrained *apagoge*, which was in turn gradually transformed into the more humane and legalistic *endeixis* with its written summons, obligatory trial, and optional arrest. Hansen, however, demonstrates that since self-help, *apagoge* with summary execution, and *endeixis* coexisted both in Drakon's law on homicide of ca. 621 B.C., and after ca. 350 B.C., they can only indicate development in Athenian criminal justice to those who press the evidence into a mould of preconceptions about "indogermanische Rechtsgeschichte." The final chapter is a catalogue of thirty-three cases where these procedures were employed in the prosecution of criminals.

These two works and his recent *Eisangelia* in the same series are parts of a large project in which Hansen aims to catalogue and analyze all trials heard in classical Athens. His emphasis on the ancient testimonia is salutary and his generalizations about Attic law are provocative. Future volumes should stimulate fresh debate on the details and principles of legal procedure in classical Athens.

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JOHN PERCIVAL. *The Roman Villa: An Historical Introduction*. (Batsford Studies in Archaeology). Berkeley and Los Angeles: University of California Press, 1977. Pp. 230. \$18.50.

John Percival attempts to assess our knowledge of an important institution around which Roman agriculture was organized in the imperial period and

on which the economy depended. His careful study amounts to a serious indictment of archaeological excavation, which has not, by and large, provided answers to the questions social and economic history asks about agriculture: what were the crops, the yield, and the rotation? What was the balance between agriculture and animal husbandry, and in what offshoot industries, such as fulling and metal working, did the villas engage? Did the pattern of agriculture and life in the individual villa change from period to period, and if so, how and why? Whether many of these questions can ever be answered by excavation seems doubtful, but perhaps there should be a moratorium declared on wholesale excavation of villas until science has devised better techniques for recovering evidence to answer the more important ones.

Villas are scattered all over the Roman world, and Percival tries to include evidence from Tunisia and Italy, together with the far more abundant material from France, the Rhineland, and Britain. But one gets the feeling that the pattern was far from uniform throughout the empire, and the villas of Northwest Europe provide the real basis of the work. These the author knows intimately; he also knows the geography and geology of their surroundings. One must have a very thorough knowledge of these parts of the world to be able to read this book without constant reference to the maps, which are provided but are not very easy to use. Even when one knows the site, one is not apt to have the road connections, character of the terrain, and quality of the soil as clearly in mind as one should to read intelligently.

The villa as a phenomenon appeared in the second half of the first century of our era, prospered and spread until the second half of the third, when invasion and upheaval brought widespread destruction and universal insecurity, and this in turn brought economic crisis. Large areas once given to villas were permanently abandoned at this time, but the majority of the western empire recovered to a remarkable degree at the beginning of the fourth and flourished for another hundred years. The great German invasions of the early fifth century mark the death of the villa system, but as this book points out, there is evidence for areas of survival in the fifth (for example, near Bordeaux), while other isolated villas became fortified castles. Subsistence farming continued on many villa sites for centuries. Others were converted or rebuilt as churches and monasteries.

The book is divided into nine chapters, beginning with definitions and our sources of evidence, continuing with a short account of the historical development and an extensive discussion of regional types (corridor villas and peristyle villas, grand estates and more modest properties). The

heart of the book poses questions about the patterns of agriculture, villa society and tenure, and villa economy and investment. It ends with the survival of the villas in changed forms. There are good brief notes, mainly references to excavation reports, bibliography, and an index of sites as well as a general index. The numerous illustrations are chiefly maps and plans.

It is a valuable book, but ultimately a distressing one. Almost every statement is tentative or heavily qualified, and the stress seems always on how little we know after a century of excavation, rather than on what we have learned.

LAWRENCE RICHARDSON, JR.
Duke University

J. W. RICH. *Declaring War in the Roman Republic in the Period of Transmarine Expansion*. (Collection Latomus, number 149.) Brussels: Latomus. 1976. Pp. 145. 450 fr.

J. W. Rich's purpose is to determine what standard procedures the Romans observed in the period 264–88 B.C. before actual military campaigns against foreign powers began. The subject is one in which much has been discussed and much assumed, but Rich is the first to assemble all that is known about the preliminaries of each war and then base general conclusions about procedure strictly on that collection.

He shows that, rigorous constitutionalism aside, the question of war was put to the *Comitia Centuriata* only in the case of overseas enemies of a fairly advanced degree of political organization in areas not already Roman provinces. A war vote and the consequent assignments of provinces and troops took place with the other annual formalities at the beginning of each consular year, but the emergence of senatorial opinion in favor of war occurred in the course of the previous year, with all but preparatory action deferred until the new magistrates took office. Rich carefully elaborates the consequences of the gap between senatorial decision and popular vote for the much-belabored preliminaries to the Second Punic War, Third Punic War, and Jugurthine War, and in each case is able to arrive at an attractive reconstruction of the sequence of events.

He then tackles the question of whether the Romans followed any standard diplomatic procedures before going to war, and concludes that they varied from war to war as the situation required: there was no regular use of the fetial procedure, no regular *res repelere*, and no regular issuance of an ultimatum. Only the announcement of war to the enemy seems to have been routinely carried out, at least until 191, but announcement to a frontier garrison would suffice. Rich's general

conclusions enable him to illuminate the particularly vexed question of the origins and purpose of the embassy to Carthage in 218, and to assert convincingly that it was not parallel in either point to the embassy to Philip in 200.

Rich has quite a few accessory points of value. He elucidates the use of terms like *res repelere* and *bellum indicere*, contributes to the prosopography of the period and to the rehabilitation of the credibility of the annalistic tradition, and tantalizingly suggests that the old fetial procedure was related in conception to the private law principle of noxal liability.

In general the book has been well edited, but it is marred by the apparent omission of a phrase of some length from footnote 192 on page 113.

JANE E. PHILLIPS
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EDWARD N. LUTTWAK. *The Grand Strategy of the Roman Empire: From the First Century A.D. to the Third*. Baltimore: Johns Hopkins University Press. 1977. Pp. xii. 255. \$12.95.

Edward N. Luttwak's title may mislead, implying as it does the discussion of war aims, of why emperors added some new areas to their empire and did not try to add still others. The author considers these questions only as they bear directly on his main subject, which is the changing shape of the empire's force *vis-à-vis* its neighbors. He distinguishes several phases, beginning with irresistably large armies by which new territories might be won or client states controlled. The latter formed a cordon which outer enemies could not penetrate without delay, delay allowing the concentration of a matching Roman response. Next, from Flavian times to the earlier third century, the absorption of client states and the construction of a permanent frontier involved not a hegemonic but a preclusive force-structure (to use Luttwak's terminology). Finally, from Gallienus on, interior mobile armies were combined in various ways with a frontier zone, to yield defense in greater or lesser depth. Underlying all these phases were quite binding considerations of disposable wealth and manpower.

Luttwak's contribution lies, first, in providing a clear and comprehensive synthesis of present knowledge about his subject. If the broad lines of his account are familiar as well as the more detailed descriptions he offers of army and frontier organization, he has nevertheless seen and filled a gap on our shelves. Moreover, he fills it in a scholarly fashion. Plain errors of fact are not very many or very important; notes and bibliography are substantial, though they reveal too little use of works in German (by A. von Domaszewski, J. Klose, D.

Hoffman, and others). Since Luttwak is self-taught (but so, after all, was Gibbon), the level of accuracy he achieves serves as a healthy reminder that the historian's profession is not a closed shop.

Luttwak's second contribution lies in his particular point of view. By profession he is a defense analyst. He therefore looks, much more keenly and consistently than other students of his subject, for the rationale and function of a military system as seen not in any one sector of the frontier but overall. He asks, how did it work; and he answers by use of large quantities of fact treated comparatively. The method is most welcome and interesting, hardly less so for being in its results quite often unpersuasive.

Its prevailing flaw seems to be one of anachronism. To begin with a trivial illustration: where Dio says Rome sent a neighbor "artisans of every craft," Luttwak speaks of "a technical aid program." The phrase calls to mind quite misleading analogies. Again, in explaining the conquest of Dacia, Luttwak contrasts the "highly rational" policy behind it with mistaken interpretations in "the light of the superficial strategy of small-scale maps"—as if the Romans had any large-scale maps of the least reliability. Much too great a degree of sophistication is attributed here to Roman planning. Luttwak infers purpose where none is attested and where the situation was beyond Roman control, as in his discovering of "the 'deep elastic' defense" where in fact the third-century Romans simply lacked the strength to prevent barbarian raids far into the heart of the empire; or in interpreting these latter as "strategic penetrations" where in fact their only aim was pillage; or finally, in seeing sense in the delimitation of salients where the lines were rather laid down by a previous occupant or by some accident. The reader senses too little patience with the primary sources, with the unpredictable peculiarities of history, and with the limits of a primitive civilization, and too great a reliance on secondary sources, on logic, and on analogy. And yet, for all the reviewer's misgivings, this remains a really interesting and stimulating book.

RAMSAY MACMULLEN
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ROGER S. BAGNALL. *The Florida Ostraka: Documents from the Roman Army in Upper Egypt*. (Greek, Roman and Byzantine Monographs, number 7.) Durham: Duke University. 1976. Pp. viii, 74. \$8.00.

The flood of papyri from Egypt has given us an enormous reservoir of information about the social, administrative, and economic life of Ptolemaic and Roman Egypt, not to mention its contribution to ancient literature. And our knowledge of

the Roman army has been made very much fuller as well. This volume offers no startlingly new material, but, like others of its kind, adds a few more colors and details to the picture. Roger Bagnall publishes here a collection of ostraka, the majority in Greek and a few in Latin, which originally came from Edfu. They are documents concerned with the day-to-day activities of a unit of the Roman armed forces; this was almost certainly an infantry unit of the *auxilia* known to us from other sources as the *Cohors I Augusta Praetoria Lusitanorum Equitata*, although there is no hard evidence in the ostraka themselves to prove the connection. The headquarters of this unit were at Contrapolinopolis Maior on the Nile. The present ostraka appear to concern a more distant outpost in the eastern desert, and they put us into direct communication with the soldiers in that outpost: a furlough pass (apparently the only example of its kind extant), many letters of both a private and an official nature, a list of watchtower guards, a list of contributors to some unknown cause, another list of guards, and a receipt for money. The most touching, from a human point of view, is also the most complete (number 14 with 22 lines), in which a soldier is very deeply concerned over the coming birth of a child to his sister in some area far from his post. He begs her to keep him informed of her progress, so that he may come to her at the proper time and help in the delivery. Other letters reflect the monotony and sense of isolation experienced by the soldiers in such outposts. Bagnall has published this material very well, even if several problems with the text and the interpretation remain unsolved in a number of places. An introduction, indexes, and excellent plates of the documents make it a useful volume for a student of the Roman army.

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H. A. DRAKE. *In Praise of Constantine: A Historical Study and New Translation of Eusebius' Tricennial Orations*. (University of California Publications in Classical Studies, number 15.) Berkeley and Los Angeles: University of California Press. 1976. Pp. xiv, 191. \$8.25.

Of the surviving accounts of the "first Christian Emperor" it is generally agreed that Eusebius' *Life of Constantine* offers "the most uncompromising testimony to that Emperor's zeal to promote the Christian faith." The connection between Constantine's private devotion and his public policies that the *Life* infers, however, has not been established to everyone's satisfaction. H. A. Drake explores this question through an examination of the

oration(s) "In Praise of Constantine" (the *De laudibus Constantini* or LC), which Eusebius, the prolific Bishop of Caesarea, produced for the celebration of the Emperor's thirtieth year in office (in 336, not 335 as others have argued). In the language and substance of the LC, Drake argues, are reflected the contours of Constantine's religious policy.

Central to Drake's thesis is his argument that the extant text of the LC in fact contains two separate orations. Eusebius delivered the first, the true LC (chapters 1–10), at Constantinople in July 336. The second (chapters 11–18), Drake believes, is the lost oration "On the Holy Sepulchre" (SC) which Eusebius read to a clerical audience in Jerusalem at the dedication of the Church of the Holy Sepulchre in 335. In support of this identification Drake cites a passage in the *Life* (iv, 46), in which Eusebius indicates that he planned to combine the LC and the SC in a single publication, and the "break" after chapter ten that occurs in all the manuscripts. The case for separation is convincing enough; more problematic is the author's interpretation of the two orations. Pointing to Eusebius' failure to mention Christ or churches in the LC, Drake concludes that the Bishop "was restrained by the official nature of the event [and] could not press the Christian interpretation beyond Constantine's limits." Instead of Christ, therefore, Eusebius emphasized the Supreme God and the Salutory Sign under which Constantine had defeated his enemies. An appeal to these divine agents was consonant with the Emperor's mission of peace, unity, and reconciliation; a direct invocation of Christ might have disturbed the pagans in the audience.

The language of the LC is undeniably conciliatory, but is it restrained? Would pagans not have recognized in Eusebius' frequent references to the "only-begotten Logos" and "our Common Universal Savior"—whom Christians must have equated with Christ—and in his tribute to Constantine's construction of religious (i.e. Christian) buildings a celebration of the Christian "triumph"? Eusebius' decision not to mention Christ or churches—introduced only in chapter sixteen of the SC—may have been less a concession to official policy than a skillful appeal to prospective converts. It was not to avoid offending the pagans that Eusebius expressed his message in the universal language of monotheism—circumlocutions in Drake's opinion—but to emphasize that the Christian God was not incompatible with the One God "enlightened" pagans worshiped. Indeed, it could be argued that the LC was intended to persuade pagans that Constantine's benevolences, far from undermining the Empire, had secured the peace both pagans and Christians enjoyed. In an Empire not yet fully committed to Christianity this is precisely what one would ex-

pect from a Christian bishop. That Eusebius recognized the advantages of this approach is demonstrated in his address to the Emperor in the SC (xi, 7): "My present account wishes to make known *to all* [italics mine] the reasons and the motives of your devout deeds." This demonstrably Christian sentiment, in my judgment, also inspired the LC.

Because Drake offers arresting assessments of a number of important issues, this is a book every student of the late Empire should read. The annotated translation of the orations alone would have justified publication.

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J. FORDE-JOHNSTON. *Hillforts of the Iron Age in England and Wales: A Survey of the Surface Evidence*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. xvi, 331. \$45.00.

After more than sixty years in which no authoritative work on hillforts has appeared, no less than three volumes have been published in the past eighteen months—in one of which this reviewer must declare a personal involvement. In fact, the treatment in each is quite different, J. Forde-Johnston's being distinctive for addressing itself principally to the surface evidence of hillforts. There has long been a tradition of field archeology—as distinct from excavation—in Britain, a tradition whose prime exponent in the interwar years was O. G. S. Crawford and which has been continued since on a professional scale by the Royal Commission on Historical Monuments for England (on whose staff Forde-Johnston briefly served) and its counterparts in Scotland and Wales. The author's close personal acquaintance with the sites on the ground is self-evident from his text. Nonetheless, we may question whether the study of surface remains, to the exclusion of excavated evidence accumulated from half a century of investigation, is really valid, a doubt which is underlined by the author's concluding discussion of chronology, in which excavated evidence is clearly fundamental.

The basis of the book is an analysis of various forms of hillforts, their topographical location and their overall distribution in England and Wales. The approach is essentially a traditional one, and no attempt is made to employ the various techniques of spatial analysis, based upon geographical models which archeologists have adopted in recent years. Equally sites are treated as entities rather than as part of integrated networks including field-systems, stock enclosures, etc., as well as the fortified foci themselves. The geographical bias of the book, towards Wessex, Wales, and the south-west, genuinely reflects the density of distribution, though it is a pity that the north of Eng-

land, and particularly Northumberland where defended enclosures abound, is almost entirely omitted.

Contrary to previous classifications, like that in the Ordnance Survey's *Map of Southern Britain in the Iron Age*, which was based upon the area enclosed and the complexity of the defenses—univallate or multivallate—Forde-Johnston's typology emphasizes the distinction between single-enclosure sites, both univallate and multivallate, and multiple-enclosure sites, his eleven types dividing into a "Wessex-tradition" and a "western-tradition." He sees both his primary types, I in Wessex and V in the west, as the product of immigration, from northern France on the one hand and from the Atlantic west, Brittany, or northwest Spain, on the other. Subsequent innovations, like the bivallate type II could result from renewed incursions, or from a native response to such movements. Though he admits also the possibility of intertribal conflict within Britain, Forde-Johnston attributes the appearance of hillforts in Britain, and their various regional manifestations, fundamentally to colonization from the Continent. The view that British Iron Age cultures were in some measure the product of intrusive populations is one with which this reviewer has considerable sympathy, but it must be admitted that this is now regarded as an outdated concept by many prehistorians who prefer to think in terms of local development from the insular Bronze Age. That hillforts were evidently already being constructed in Britain in the late Bronze Age, if not in the later second millennium, might be taken to support this view. In fact, the "short chronology" to which Forde-Johnston refers was already obsolescent fifteen years ago, and the reappraisal of hillfort origins in Britain was inevitable, not only in consequence of the updating of the Deverel-Rimbury culture, but as a corollary of research in central Europe, where hill fortifications can now be seen to develop from early Bronze Age antecedents.

This handsome book is the product of long and patient work over a number of years, and we are indebted to the author for his labors. It is not, of course, his fault that this fundamental analysis of surface remains was not written forty years ago, when modern hillfort excavation was beginning; but, as he himself ruefully implies, that is when it was really needed.

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MEDIEVAL

JEAN GIMPEL. *The Medieval Machine: The Industrial Revolution of the Middle Ages*. New York: Holt, Rinehart and Winston. 1976. Pp. xi. 274. \$12.95.

Jean Gimpel presents two themes in his book. The first argues a Spenglerian thesis that Western technology peaked in 1971 and that the Occident (read the United States) is now in decline. The second theme states that the Middle Ages was a dynamic and innovative period which deserves to be known as the first industrial revolution. This book is aimed at a popular audience, and it is perhaps symptomatic of our age that it will be read largely for the first thesis rather than for the second. Major portions of the chapter expounding the decline of the United States have been reprinted in a journal of public opinion. This is unfortunate because it is the second thesis which should be more widely understood by the educated public. Despite decades of scholarly research which shows the Middle Ages to have been innovative and dynamic, the "media" view of this era as the Dark Ages still prevails.

By interweaving the two themes in the same volume, Gimpel clouds his intent. His stress upon such words as "decline" and "revolution" do not help to clarify events. At one point, Gimpel outlines what at first glance is an interesting list of similarities between the United States in decline and the failure of the medieval technological revolution in the fourteenth century. Upon reflection, however, the reader is drawn to look for exceptions. In the modern period, for example, the author ignores space exploration and what it has meant for medicine. For the medieval era he emphasizes military engineering in late medieval technological manuals, viewing this as an element of decline, but other developments found in them, such as machine design, metallurgical processes, and innovations in building construction, were more important. For the specialist, the arguments and examples Gimpel presents can stimulate as well as infuriate. For those without a background in the history of technology, the danger of misconception is heightened unless they are spurred to read further.

Gimpel includes no bibliography, but a substantial number of works on medieval technology and economic history are listed in the footnotes. Yet, one is struck by what is excluded. Sometimes he indicates his sources, but on other occasions he leaves the reader in a quandary about the origin of his material.

Not all of the differences between the French and the English editions of this book are felicitous. On page viii of the English, he states that "scientists and engineers of that time were searching for alternative sources of energy to hydraulic power, wind power and tidal energy." He offers no explanation of this statement, and certainly it was not the alternatives to, but the use of these three forms of energy which provided the basis for a dynamic technology. Gimpel points to manuscript

illuminations of architect-engineers in the thirteenth century as a significant psychic development, but this theme can be found in the late Anglo-Saxon period. Gimpel tends to draw too close a connection between academic science and the technologist. He describes medieval science as "experimental" where "experiential" would have been a better term. The use of the terms "socialist and communist ideals" to describe proletarian rebellions in the late Middle Ages is debatable.

Such questions of interpretation and some errors of fact make this a less than satisfying book. Yet, the author does whet our appetites. Many of the examples he cites will be new to some readers. His discussion of the high quality of the medieval diet and his account of Bazacle Dam on the Garonne, which led to the creation of the oldest limited company in Europe, do express the dynamics of this age. Despite considerable shortcomings, Gimpel offers much that is informative, but a more thoughtful study of the implications of medieval technology is still necessary.

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HEINRICH FICHTEAU. *Beiträge zur Mediävistik: Ausgewählte Aufsätze*. Volume 1, *Allgemeine Geschichte*. Stuttgart: Anton Hiersemann. 1975. Pp. viii, 309. DM 96.

Heinrich Fichtenau, director of the Institute of Austrian Historical Research at Vienna, is not only one of the leading European medievalists, but also one of the foremost general historians of the Middle Ages. His famous book *Das karolingische Imperium* (1949), which appeared in an English translation by Peter Munz under the title *The Carolingian Empire*, presented a new, critical approach to Carolingian studies. It was followed by significant publications of medieval charters and various books and monographs dealing with sundry subjects of historical diplomatics. His most recent work shows his labors in other spheres of the wide field of medieval history.

The present collection of studies, some of which have previously appeared either as monographs or as articles, testifies to the great sensitivity of Fichtenau for the understanding of complex mentalities of individuals and of the political, cultural, and religious concepts of groups and systems.

The series of studies opens with a knowledgeable analysis of ancient Roman history in the works of German chronicles and chroniclers. The reprinting of Fichtenau's famous monograph on "Asceticism and Vice in the Thinking of the Middle Ages" will be welcomed by everybody who knows this brilliant investigation. It offers on the one hand a

fine characterization of asceticism and its changes in time and space, and on the other hand the doctrinal concepts of vice and the human involvement and opposition against its manifold manifestations. Equally valuable and instructive is the investigation of the difficult subject of the nature and the characteristics of religious relics of the earlier Middle Ages, a study that probes deeply into the medieval mind and its social thinking. Middle Latin philology is represented by the article on the recitative prose of the high Middle Ages. "Outsize Bibles in Austria and Mathilda of Tuscia" deals with the paleographical transmission of giant Bible manuscripts.

Biographical studies of Austrians and Frenchmen delineate the degree to which the medieval world was consciously known to, and understood by, its contemporary beholders. "Magister Petrus of Vienna" describes the little-known life and important activities of a personality significant in an intellectual profile of the court of the ruling house of the Babenberger at Vienna. "Akkon, Cyprus and the Ransom for Richard the Lion-Hearted" leads us into the ramifications of an international aspect of Austria's medieval history. This is equally true of "Empire and Dynasty in the Political Thought of Emperor Maximilian I," which brings us forward to the transition from the Middle Ages to modern times. The last study treats the personal tragedy of a nineteenth-century scholar and his alleged literary forgeries, which are analyzed by Fichtenau in a humane and very noble manner.

While the first volume of these selected studies clearly deals with sundry historical mentalities, the second volume will be largely centered on documentary and diplomatic studies.

LUITPOLD WALLACH
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JOSEPH H. LYNCH. *Simoniacal Entry into Religious Life from 1000 to 1260: A Social, Economic and Legal Study*. Columbus: Ohio State University Press. 1976. Pp. xix, 266. \$15.00.

This clearly written and accurately documented study begins with a consideration of the endowment structures of monasteries and of the delicate problem of balancing enrollments against resources. A related problem it treats arose from the fact that few donations gave clear, undisputed title to the recipients; thus few religious houses were not engaged in several property disputes at any given time. The negotiations that preceded the entry of an individual into a monastery concerning the size of the gift he would bring with him responded to both these problems: they served 1.) to

assure both parties that the community would be able to support the individual, and 2.) to settle outstanding property disputes between the community and the entrant's kin.

The author's inquiry focuses on a spiritual and legal issue, namely the propriety of such gifts. Entry gifts and the negotiations concerning them were standard practices whose legitimacy was called into question only in the 1120s. The author examines successively the early criticisms of these practices, the formal definition of them as simony, the disciplinary measures taken against them by the papacy and the episcopate, and the reform measures provided by the religious orders themselves.

Of the three disciplinary approaches announced in the subtitle, the third—legal—is the one carried out most successfully. While social and economic matters are indeed mentioned, the historical problem of simoniacal entry into religious life is not subjected to social or economic analysis. The absence of such analysis is unfortunate for it might have supplied the author with an answer to the most pressing question his study raises, to wit: Why was a well established, traditional practice called to account at a particular time? The author's lone, direct confrontation with the issue is unintentionally evasive (p. 98): "There was nothing inevitable in the development that led to payments at entry being classified as simoniacal. The issue could have been ignored, as it had been for centuries, or could have continued to be classified as 'filthy gain' or as a spiritual danger to the new monk. However, the twelfth century was acutely aware of the problem of simony, and it was natural for entry payments to be seen from the perspective of payments for a holy thing."

Why was it not natural before, and why was the problem of simony not so acutely perceived before? One answer is suggested by the importance of gift-exchange practices in the European economy up to about 1050. Prior to that time, no major social, political, or spiritual act, no rite of passage, would not have been marked by the distribution and exchange of gifts. Beginning in the mid-eleventh century, particularly in those areas with a precocious commercial, profit-calculating economy (such as those that spawned Peter Damian, Hildebrand, and Humbert of Silva Candida), certain "gifts" began to be perceived as bribes, even though a centuries-old vocabulary (*simoniaca heresis*) was brought back into use to express this perception. The issue of the propriety of payments at entry could not have arisen before the eleventh century and could not have been avoided thereafter.

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ALAIN DUCELLIER. *Le drame de Byzance: Idéal et échec d'une société chrétienne*. (Le Temps et les Hommes.) Paris: Hachette. 1976. Pp. 318. 49 fr.

Despite enormous advances in scholarship over the past century, Byzantine civilization still remains largely misunderstood, particularly among the general public. In part this is because of intractable prejudice, but it is also a result of the failure of Byzantinists to ask many of the significant questions current in other fields of historical inquiry. It is therefore with considerable anticipation that one reads Alain Ducellier's promise that *Le Drame de Byzance* will focus on the *mécanismes mentaux* of Byzantine civilization.

The first section of the book deals with a number of related aspects of Byzantine social life: the family, the place of women, personal freedom, and the survival of the classical tradition. The second section considers imperial theory and practice, while the final section examines Byzantine spiritual life and the wide continuum from traditional orthodox Christianity to crude superstition.

Throughout the book Ducellier stresses the contrast between the ideals of Byzantine civilization and the realities which frequently fell far short of them. Thus, he contrasts the nearly absolute authority of the *basileus* with the frequent coups against the throne and the ideal of harmonious family life with ever-present infidelities and brutality. Ducellier raises an interesting point by suggesting that Byzantium, unlike most other comparable societies, did not compromise the integrity of its ideals in the face of human weakness. On the contrary, individual failures were judged as essentially culpable, and this tension produced the peculiar mentality characteristic of Byzantine civilization. Indeed, Ducellier argues, Byzantine mentality survived the political collapse of 1453 and continued in the East—as exemplified by such enigmatic personalities as Mitia Karamazov and Joseph Stalin.

This is an interesting and thought-provoking book, but ultimately much of its analysis fails, either because it is obvious or because it is wrong. It is hardly useful to explore once again the tensions inherent in Byzantine imperial theory unless the analysis goes beyond what has already been said many times over. It would have been interesting, for example, to learn how Byzantine theory was actually received by contemporaries, but all the book provides is the classical theory of the "right of revolution." This example could be multiplied, and in the end the reader does not really learn much about the Byzantine mind: it was torn between ideals and realities, but such a situation is hardly reserved to Byzantium.

Part of the difficulty lies in the scope encom-

passed by this book. Because of the state of our knowledge of Byzantine life, many small-scale studies will be necessary before a work as ambitious as this can profitably be written. Further, as Ducellier is aware, Byzantine civilization was complex and it changed considerably over time. Although he proposed to limit his consideration to the period of the "high empire" (seventh through twelfth centuries), Ducellier frequently illustrates his major points with examples taken from an earlier or a later period.

On some topics Ducellier's treatment is particularly good—for example, his analysis of Byzantine views toward foreigners—and it might have been better if he had confined his study to a few related matters. One must encourage the kind of analysis suggested by Ducellier, but for the present the older and less ambitious syntheses of Byzantine civilization (Brehier, Hussey, Baynes, and Moss) will have to suffice.

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JEAN-NOËL BIRABEN. *Les hommes et la peste en France et dans les pays européens et méditerranéens. Volume 2, Les hommes face à la peste.* (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques, Civilisations et Sociétés, number 36.) Paris: Mouton. 1976. Pp. 416.

Jean-Noël Biraben's second volume on the history of plague has all the strengths and weaknesses of the first. This volume deals with the attitudes, ideas, and behavior of those who actually confronted the plague.

Biraben proves that, without a theory of contagion, most European observers were bewildered by the comings and goings of the plague. Many believed that disease was a form of divine vengeance. Some chroniclers, like the author of the fourteenth-century *Tractatus de epidimia*, found the answer in the stars. At certain moments, popular apprehension over the plague turned to violence as blame was laid on Jews, lepers, or sorcerers.

Biraben aptly illustrates the helplessness that most felt in the presence of the plague. The exaggerated belief that a sick person could infect someone who was healthy by a stare was common. So was the suspicion that domestic animals might be the source of the disease. There were as many theories surrounding the cause of plague as there were outbreaks. What all of this suggests is that, in the absence of scientific knowledge, imagination about cause and cure had full reign in this superstitious period.

Although most chroniclers were unable to explain the origins of plague accurately, public authorities did try to contain it. Here the human

record improves. Clinical observations of the disease became more empirical. Di Coppi Stephani's fourteenth-century tract precisely describes the pneumonic form of plague that struck Europe so devastatingly. Armed with this type of knowledge, public authority responded energetically to these outbreaks. In fact, one of the major themes to emerge from Biraben's second volume is that not only did plague advance the cause of public health, it also advanced governmental power significantly. Everywhere in Europe, governments passed legislation to deal with the problem of plague. Ordinances appeared governing burial and the care of the poor and limiting commercial activity. Army and police units were used to detect the disease. Health departments were set up with regulatory powers. None of these measures actually checked the spread of plague, but they did make government stronger and more intrusive.

Throughout both volumes, Biraben uses extensive documentation. His bibliography alone is 227 pages long. As a feat of research Biraben's study is an extraordinary contribution. Still, questions persist about this very workmanlike synthesis. It is still unclear, after two volumes, who most often died from the plague: the rich, the poor, or those in-between. Biraben's demographic knowledge badly needs to be interpreted sociologically. And there is another question raised by his study. Can historians of disease continue to work without a background in biology, in this case bacteriology? The history of disease is coming of age. It can only become truly meaningful, however, if it is aided along the way by sociology and biology.

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K. P. WITNEY. *The Jutish Forest: A Study of the Weald of Kent from 450 to 1380 A. D.* London: Athlone Press; distributed by Humanities Press, Atlantic Highlands, N. J. 1976. Pp. xvi, 339. \$20.00.

The nineteenth-century historian, E. A. Freeman, pictured the defeated Anglo-Saxons after the battle of Hastings as disappearing into the pathless forest of the Andredsweald. If he could have visited the Weald of Kent and Sussex at this time he would have found many paths to guide the remnant of Harold's army toward London and the Thames valley. Yet the Weald was indeed a forested region. Its core is made up of low hills whose sterile soils have remained largely wooded into modern times. These are framed by a belt of clay, heavy and intractable, which also defied the efforts of early settlers to occupy it. Lastly, a sandstone ridge, called the Chart Hills by the author, forms the boundary of the Wealden region, beyond

which to the north and south the chalk downs provided an open countryside for the early settlers and a light soil for them to cultivate.

The wealth of the Weald lay in its scattered deposits of iron ore and its abundant woodland in which the oak was common and locally predominant. The Romans had exploited the iron and in doing so had cleared small areas and built roads to export the metal. But late in the Roman period iron-working was abandoned and not revived until late in the Middle Ages. During the intervening thousand years, with which this book is concerned, the resources of the region were exploited differently: for fuel, for construction timber, and above all for pannage.

This intermediate period began with the invasion of southeastern England by the Jutes. Their institutions, as is well known, differed from those of the Anglo-Saxons, and strongly influenced the ways in which the region was exploited. Their settlements were primarily on the good soils of the north, but appendant to these were rights of common in the unsettled and forested Weald. The latter provided pannage each fall for the hogs which were driven to the oak woods from the more populous areas in the north. The administrative units of early Kent, the lathes, and also their successors, the hundreds, were so drawn that each embraced both an area of good land and also an extent of Wealden forest. One of the many achievements of this book is to work out the detail of this administrative structure and to trace the drove roads along which the herds made their slow autumnal progress to feast on the harvest of acorns.

K. P. Witney traces in great detail but with admirable clarity the rise of this aspect of the medieval economy, relating it to population growth and to the emergence of other claims on the forest resources, until finally the "dens" were transformed into fully agricultural settlements. He makes great and effective use of place-names, though much of this evidence is fortunately banished to appendixes. This study is a valuable contribution to the history of man's impact on his environment, its value heightened by the many maps. It is a model for other studies of this kind.

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ELISABETH MAGNOU-NORTIER, *La société laïque et l'église dans la province ecclésiastique de Narbonne (zone cispyrénéenne) de la fin du VIII^e à la fin du XI^e siècle*. (Publications de l'Université de Toulouse-Le Mirail, series A, number 20.) Toulouse: Association des Publications de l'Université de Toulouse-Le Mirail. 1974. Pp. 685. 95 fr.

Arising from an interest in the impact of the Gregorian reform movement upon southern France, this historical inquiry by Elisabeth Magnou-Nortier of the University of Picardy is a monumental examination of secular and ecclesiastical society in the archdiocese of Narbonne. This study is built on an intimate knowledge of the manuscript collections of the Midi and relevant documents from other archives outside this region. But, as the author indicates at the beginning of her essay, the nature of the source material creates endless problems. First, most of the evidence is the product of the activities of the elites of the archdiocese; nothing survives from the peasant stratum of the society. Second, much of the material relates to ecclesiastical institutions, especially the monasteries, thereby warping the picture of the secular aristocracies. Third, no governmental archives of the counts of Toulouse, the secular rulers of the area, exist. To some degree these lacunae may be filled indirectly from the surviving sources. In part this is achieved by a careful examination of the changes in terminology employed in the charters, royal acts, papal bulls, law codes, and other documents.

In comparison to the north of France, the Midi of the period was more Romanized and urbanized, with fewer elements of feudalized lordship present. Ancient Roman institutions had influenced the development of governmental practices so that the comital government was based upon public institutions rather than private (i.e., feudalized) contractual relationships among the landed aristocracy. Because of the retardation of feudalism meridional society tended to be horizontal rather than vertical in its class relationships. This was particularly true in the aristocracy, which ruled through public governmental institutions. The secular government was aided by the Church's desire for peace. The archdiocesan clergy, using the Peace of God and their great wealth and considerable prestige, did much to maintain law and order in the Midi. The success of the Church in this regard, coupled with its religious role, had reduced the secular leaders to a secondary position by the end of the eleventh century. Until then the prelates and the upper aristocracy had worked in concert in ruling the Midi. The papal-induced Gregorian reforms accentuated the differences between the ecclesiastical and secular ruling orders that resulted in their mutual alienation. The aristocracy's secondary position and its loss of control of local ecclesiastical institutions seem to have made it ripe to receive and act upon Urban II's call at Clermont.

While the meridional free peasantry appears to have been in a better position than its northern cousins, in the eleventh century its position before

the law was coming to be more closely defined. The definition of the *coutumes* may have been as much an effort by the peasantry (free and serf) to protect its rights as an attempt of the aristocracy to extend its control over the free peasantry. Like the aristocracy, the peasantry was stratified horizontally, with the upper orders involved with the ruling elements in the towns and countryside.

Magnou-Nortier thinks that Jacques Flach's view that the *regnum Francorum* was divided into great ethnic territories ruled by grand magnates is a correct picture of the county of Toulouse. The usual feudal institutions are not to be found there. The counts ruled through a combination of public powers and lordship. Nor did the counts see themselves as vassals to the king of France. Instead they thought of themselves as equals to the anointed monarchs, be they kings of France or Byzantine emperors.

To summarize Magnou-Nortier's findings is to do her an injustice, as generalizations overly simplify her intricate portrait of the archdiocese. She has provided a careful analysis of its social and governmental institutions. While quite concerned with local practices, she has not failed to relate her conclusions to wider developments in Western Europe. Her meticulous analysis, combined with the numerous maps, tables, lists, and indexes, makes this study a pleasure to read and use.

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JACQUES BOUSSARD. *Nouvelle histoire de Paris: De la fin du siège de 885-886 à la mort de Philippe Auguste*. Paris: Association pour la publication d'une Histoire de Paris; distributed by Hachette, Paris. 1976. Pp. 439.

Paris always fascinates. Jacques Boussard's contribution to the *Nouvelle Histoire de Paris*, five volumes of which have already appeared, takes up the story of the city at a critical juncture—its transformation from a rural *bourgade*, left in semi-ruins by the Norman siege of 885-86, into the political, military, and cultural capital of France by the end of the reign of Philip Augustus (1223).

From earliest times, the core of Paris was the *île* itself, divided between king and bishop who disputed hegemony over the city. River life dictated the direction and pace of urban growth; it caused original settlement to develop on an east-west axis, running on the right bank from the *bourg* Saint-Germain-l'Auxerrois in the west to the *monceau* Saint-Gervais on the east. In between lay the commercial heart of early Paris, the natural port of la Grève, the principal *porte de Paris* and, as Lombard-Jourdan has recently shown, the symbolic center of the city with a rich heritage from pre-Roman

times. The left bank, although more sparsely populated, was similarly constituted by a triad of *bourgs*: Saint-Germain-des-Prés, Sainte-Geneviève, and Saint-Marcel. It is the great merit of Boussard's book to trace, with the highest degree of precision permitted by the surviving sources, the evolution of Paris from this series of more or less independent *bourgs* into an integrated *ville*.

In this evolution two groups played leading roles: the bourgeois inhabitants of the right bank, forming the *cellule génératrice* of urban progress, and the scholars of the left bank, contributing population for that otherwise largely rural sector of the city. Boussard sensitively describes the interplay between commercial and cultural factors which provided the chief impulse to expansion, the complex relations Paris maintained with its *banlieue*, and the struggle between king and bishop for control over the riches the city created.

Inevitably, one will find things to quarrel with in a book of this scope. For example, can one really speak of a "renaissance" of Paris in the tenth century? Should not Mathew Paris' report of a corporation of masters in Paris as early as 1170 be construed as a reading back into earlier times of conditions familiar to him in the mid-thirteenth century? Similarly, one may question whether the right of Sainte-Geneviève to grant the *licentia docendi* continued to play as important a part in the evolution of the University at the turn of the twelfth century as Boussard suggests.

Although a "new history" of Paris, Boussard's work happily eschews urban theorizing in favor of a scrupulous attention to documentary remains. The book is handsomely printed and richly illustrated and will delight those for whom Paris remains, as Ernest Hemingway once called it, a moveable feast.

GABRIELLE M. SPIEGEL

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GUY BOIS. *Crise du féodalisme: Économie rurale et démographie en Normandie orientale du début du 14^e siècle au milieu du 16^e siècle*. (Cahiers de la Fondation Nationale des Sciences Politiques, number 202.) Paris: Presses de la Fondation Nationale des Sciences Politiques. 1976. Pp. 410. 105 fr.

Guy Bois has written an important book on the evolution of French society and economy from the thirteenth to the mid-sixteenth century that should stimulate rethinking in a number of areas on the nature and functioning of medieval rural society. For Bois, the crises of the fourteenth century—wars, plagues, climatic changes, population losses, and economic instability—were incidental to a fundamental crisis inherent in the very structure of

"feudal" society. He has sought therefore to isolate the internal forces and long-term inter-relationships between economic, social, and demographic changes in eastern Normandy, where abundant documentation is available. As the title indicates, this is not a local study in the traditional sense; Bois is interested in the particular only in so far as it contributes to our understanding of the still largely unknown social and economic mechanisms of preindustrial Europe.

The care with which this study was conceived and executed is apparent in its organization and method of analysis. The first of the three independent sections of the book is a macroanalysis of demographic, price, and wage movements in which each quantifiable source is evaluated critically before its information is extracted (the long section on the registers of hearth taxes and the distinction between family and fiscal hearths is especially good). Most interesting are the resultant correlations of high population and high grain prices (ca. 1250–1330, 1460–1550) and of low grain prices, population decline, and high labor costs (1330–1460). The staggering fifty percent population loss in one generation (1348–74) agrees with recent findings from other areas.

The second section is a microanalysis of several villages, peasants, wage laborers, and great landlords; it provides a close look at Norman society at the lowest level. Given the technology of the time, the most efficient scale of farming was the single family unit of the average peasant. Since most landlords had ninety percent of their tenant rents as perpetually fixed cash payments and were dependent on costly wage labor to cultivate their own reserve lands, they suffered a steady decline in real income, in some cases up to seventy percent from 1314 to 1450. At the same time peasants and artisans were improving their standard of living. The third section of the book fits the short-term secular trends into a detailed chronological synthesis.

Bois has turned "feudalism" on its head by defining it as a society in which the small peasant farmer dominated its most important economic activity, the production of grain. Landlords were simply tax collectors who had no interest in increasing production itself; their reliance on fixed rents, which were detached from rising grain production and prices, created the central crisis of declining aristocratic revenues. This book, rich in information and ideas, is a welcome contribution to the general re-examination of "feudal" society now under way.

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GÜNTER RAUCH. *Pröpste, Propstei und Stift von Sankt Bartholomäus in Frankfurt: 9. Jahrhundert bis 1802.*

(Studien zur Frankfurter Geschichte, number 8.) Frankfurt: Waldemar Kramer, 1975. Pp. 415.

This is not a history of the coronation church of St. Bartholomew's in Frankfurt, but rather an investigation of the procedures used in selecting its provosts. This topic was only briefly covered in Wolf Erich Kellner's *Das Reichsstift St. Bartholomäus* (1962). The archbishops of Mainz, who acquired the Carolingian palatine chapel in Frankfurt in the eleventh century, possessed the right of collation. To counter growing papal interference in the selection process, the chapter usurped the right to elect the provost in 1373. Charles V's protection assured the survival of the collegiate church during the Reformation, but in return it was forced to accept imperial presentation. Rudolph II conferred on the canons once more in 1582 the right of election, and they successfully asserted it, in spite of curial opposition, until the secularization of the foundation in 1802.

While Günter Rauch must be commended for his meticulous scholarship, the effort is out of proportion with the significance of the topic. The provosts were for the most part absentee pluralists who paid little attention to the church. The nature of the extant evidence has forced Rauch to focus on the early modern period rather than the Middle Ages, but St. Bartholomew's can best be described in the post-Reformation era as a moribund institution, totally isolated from the hostile Protestant city in which it was situated. Rauch's discussion of the disputes surrounding the filling of a lucrative sinecure thus provides little information either to the local historian or to the medievalist interested in collegiate churches. The secular canons deserve more attention than they have hitherto been given by scholars, but this book does not meet the need. A far more useful approach would have been to examine several collegiate churches in a given time period and area, such as in the archdiocese of Mainz, and to compare their development. One cannot help but be envious that a number of Frankfurt banks and corporations were willing to finance the publication of this monograph; one can only hope that their American counterparts might be persuaded to copy their example.

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WILTRUD EIKENBERG. *Das Handelshaus der Runtinger zu Regensburg: Ein Spiegel süddeutschen Rechts-, Handels-, und Wirtschaftslebens im ausgehenden 14. Jahrhundert.* With an appendix by WALTER BOLL. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 43.) Göttingen: Vandenhoeck and Ruprecht, 1976. Pp. xx, 336. DM 78.

An account book kept by Wilhelm and Matthäus Runtinger, merchants and patricians of Regensburg, provides the basis for this monograph. Wilhelm began the book in 1383. On his death in 1389 his son Matthäus continued it until 1407, when the male line of the family died out. The book was rediscovered in 1893 and edited in 1935 by Franz Bastian. Wiltrud Eikenberg acknowledges her debt to Bastian and seeks to provide what he did not: a satisfactory and complete picture of Runtinger activity as contained in the book. The base is narrow but serves the monographic nature of Eikenberg's study well.

The first five chapters of her study (on the account book itself, the Runtinger family, and life in fourteenth-century Regensburg) provide the background for the three major sections of the monograph: on the commodities traded and markets visited (chapters 6–9), on business organization and methods (chapters 10–13), and on the landed interests of the family (chapters 14–16). The value of this study lies in the detailed information in these sections; it is strengthened by the clear historical and critical context the author provides.

The Runtinger were especially active as a commercial link between Venice and Prague (later Vienna). Each fee that had to be paid, each measure or weight that was used, each toll station passed along the way is discussed, wherever possible. Other items are gleaned from the account book. Matthäus knew Arabic numbers; his wife had the necessary writing and mathematical skills to help conduct the business; and so on. This concrete treatment of detail allows Eikenberg to establish that South German trade was not hopelessly backward in comparison to Italian and Hanseatic trade in this period.

She establishes the same point for business organization. The account book provides the only detailed information on South German organization before 1400 and the only account of business on commission in the region before the end of the fifteenth century. Wilhelm and his son had an ongoing partnership, but it was not a family firm. They used both salaried agents and those who worked on commission. In both cases the agents had broad authority to conduct transactions on their own, and at least two of the salaried agents later became merchants themselves.

An appendix on currency, weights and measures, and some comparative prices completes Eikenberg's study. Then comes another appendix, by Walter Boll, on the house inhabited by the Runtinger, followed by maps and plates. In between is the index, but fortunately the table of contents is so detailed that one does not have to consult the index often.

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MARIO AMELOTI and GIORGIO COSTAMAGNA. *Alle origini del notariato italiano*. (Studi Storici sul Notariato Italiano, number 2.) Rome: Consiglio Nazionale del Notariato. 1975. Pp. xiv, 346.

This volume is the second in the series "*Studi Storici sul Notariato Italiano*" sponsored by the *Consiglio Nazionale del Notariato*. In a long introduction Mario Amelotti surveys the public and private documents of the Roman Empire and the Byzantine world of Ravenna and south Italy to distinguish the strands of their history relevant to the evolution of one of the most fundamental institutions of medieval Italy, the notarial act. The story is then taken up by Giorgio Costamagna, and the bulk of the book is his survey of notarial practice in the early Middle Ages through the eleventh century. By that time a tradition of recourse to notarial acts was so thoroughly established that the notary was able to survive the demise of the Carolingian empire and the political system from which he had originally derived his authority. By adapting the instrument to new conditions, especially those connected with the rise of towns, he assured the survival of the notarial instrument as a means of securing contracts.

Costamagna is a specialist in diplomatics, and he continually emphasizes the difference between his approach and that of legal historians, who have been more concerned with the relevance of documents to the larger legal structure. He concentrates on the document itself, wringing from it every piece of internal evidence in order to put it in the fullest social and cultural context. He is concerned with the identities of the parties involved in making contracts and the authority recognized in drawing them up. The central chapters survey the formulas used in the notarial documents—to explain their remarkable uniformity and to demonstrate their vital function in establishing the document's essential nature, its credibility. His study is a model of its kind, exhaustive in its survey of the documents and dispassionate, thorough, and wide-ranging in its critical review of the literature on the subject. It takes us much farther into the world that generated these documents than do the more legalistic treatments of the subject.

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ANTONIO IVAN PINI. *La popolazione di Imola e del suo territorio nel XIII e XIV secolo*. Bologna: Pàtron Editore. 1976. Pp. 211. L. 5,500.

Antonio Ivan Pini, though not the first to analyze the demographic history of thirteenth- and fourteenth-century Imola, one of the few Italian communes of this period for which suitable sources have survived for the rural territory as well as for

the city itself, has provided the most thorough and imaginative investigation of the subject.

Pini's thesis, based upon his detailed demographic curve for Imola, its dependent *distretto* and the "independent" Imolese *contado*, is that the population pattern of Imola and its *distretto* is similar to that found for other communes, but that the pattern for the Imolese *contado* departs significantly from the traditional picture. The author estimates that the population of the city was approximately 4,200 in 1210, that it increased throughout the thirteenth century, and reached its apogee, 11,500 inhabitants, in 1312. By 1336 it had declined to 8,800 and had further slipped to 5,350 inhabitants by 1371. For the *distretto*, that section of the Imolese *contado* extending seven or eight kilometers from the urban center and directly under Imola's political and fiscal control, Pini finds a similar demographic curve of thirteenth-century growth and fourteenth-century decline. For the politically independent *contado*, however, Pini reaches conclusions that are startlingly unique in comparison to what is known for the rest of communal Italy. He contends that the population of the *contado* in 1371, which he estimates at 12,965, was greater than it was in 1265 (12,100 inhabitants), and hypothesizes that this "revival" of the *contado* population was the result of "inverse urbanization," a flow of people from city to country. Further connecting this migration with the "return to the land" phenomenon of the fifteenth and sixteenth centuries, which he believes he has shown may actually have begun much earlier at Imola, Pini suggests that the key to this "inverse urbanization" lies in Imola's peculiar political development and the failure of the commune to gain control of the Imolese *contado*. With its base restricted to its *distretto*, according to Pini, Imola's population level could not be maintained.

In general, Pini handles his sources with great skill and ingenuity, but there are curious lapses in his technique. For example, he utilizes guild records of 1272 to estimate Imola's population by using the membership lists as the base figure and the known ratio of artisans to total population of neighboring Bologna as the coefficient. He ignores the evidence available in Lerner which shows that the Imolese guilds at this time did *not* reflect economic reality, but were actually *political* units dominated by the feudal nobility.

Yet, even if one accepts all Pini's demographic data, one still wonders if his evidence is substantial enough to support his interpretations. Where is the evidence of population *increase* in the fourteenth century? All Pini has are data from two dates: 1265 and 1371. How can one assume that the larger and later figure is the result of later fourteenth-century growth? Might the pattern of the *contado* not be like that of the city and *distretto*, i.e., increase in the late

thirteenth and early fourteenth century, with subsequent decline in the later fourteenth century? The two dates are simply too remote from each other to enable one to speak of a "trend" as Pini does. Moreover, he offers no explanation for the contrast between the population pattern of the *distretto* and *contado*. Would not the *distretto*, the rural area closest to Imola, have felt the effects of "inverse urbanization" sooner than the more remote *contado* areas? But, according to Pini's own figures, the *distretto* shows a decline in the later fourteenth century.

Nevertheless, Pini has given us the most complete analysis to date of Imolese demographic materials for the communal period and has offered some provocative suggestions for interpreting those materials. Although he has not proved his thesis conclusively, he has pointed to whole areas of communal history and demography which deserve further investigation.

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BENJAMIN Z. KEDAR. *Merchants in Crisis: Genoese and Venetian Men of Affairs and the Fourteenth-Century Depression*. (Yale Series in Economic History.) New Haven: Yale University Press. 1976. Pp. xii, 260. \$17.50.

Benjamin Z. Kedar contends that Genoa and Venice experienced a commercial depression beginning in the middle of the fourteenth century, that this depression affected the "mood" or mentality of Genoese and Venetian merchants, and that the shift in mood led them to "over-react" and become unduly conservative as merchants. His findings about their "mood"—the most original part of his work—contribute modestly to what has been learned by Huizinga, Meiss, Bec, Tenenti, Trinkaus, and others about the outlook of Europeans during the "waning" Middle Ages.

Concerning the commercial depression Kedar presents evidence drawn from very wide reading in published and archival sources and historical studies. His evidence, however, does not always support his conclusions. For example, the prices paid in Genoa for the right to farm customs duties and in Venice for government bonds are poor indicators of market activity. Furthermore, his graphs of these proxies show opposite trends in the two cities. His data on Genoese customs payments in 1376 and Venetian taxable wealth in 1379 do not prove his point about changing opportunities to become merchants. (And incidentally the labels on the axes of the graphs are reversed.)

Kedar maintains that after the mid-fourteenth century Genoese and Venetian merchants became convinced of the wretchedness of the human condition, saw Fortune as a major and usually dangerous force in life, and adopted the virtues of

prudence and circumspection. They also lost faith in man's capabilities and therefore felt more dependent on a God who, they now believed, often intervened miraculously in human affairs.

What caused the shift in "mood"? Kedar attributes it chiefly to the commercial depression. At some points, however, he suggests that the new mood may have been partly a response to plague epidemics, civil strife in Genoa, or the long war between Venice and Genoa.

Though it is clear that Kedar searched imaginatively for evidence of the change of mood, much of what he found is not convincing. For example, the new awareness of time, on which he has interesting material, does not appear to me to reflect "norms of prudence and circumspection." On the increasing use of saints' names for children, Kedar's own evidence indicates that while a spurt in this usage did happen in Genoa during the commercial depression, "from a very early time, most Venetians were called after a saint." And Venetian writers' frequent use of two vaguely synonymous words where one would do (for example, "*statuendo e ordinando*") is not a "further manifestation of the decline in human self-confidence" (p. 108), but an ancient rhetorical device.

In a brief final chapter Kedar suggests that the merchants over-reacted to the commercial depression and became timid and conservative. To judge by his own account, however, the two groups of merchants showed considerable vitality in trade, piracy, and their war against each other.

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ALAN RYDER. *The Kingdom of Naples under Alfonso the Magnanimous: The Making of a Modern State*. Oxford: Clarendon Press. 1976. Pp. viii, 409. \$26.25.

This is a virtual encyclopedia of the government of Naples from 1435 to 1458. After a survey of the Italian and Spanish antecedents of Alfonso's administration, Alan Ryder examines each of the major institutions of government, from the king and his vassals down through the royal household, the councils and parliaments, justice, finance, secretarial corps, military, and provincial administration. In each of these sections, the author examines the juridical foundations of the institution, its structure, procedures, personnel, salaries, expenses, taxes, jurisdiction and competence, and the degree to which it met Alfonso's expectations. The wealth of detail is somewhat daunting, but it is invaluable to have collected, in one meticulously reliable work, information on everything from the customs tax paid on animals leaving the Kingdom of Naples to the duties and perquisites of the personnel in the royal chancery.

The book is based on a previously unexploited

source, the Neapolitan documents deposited in the *Archivo de la Corona de Aragón*, which the author has used to excellent advantage in conjunction with better known contemporary accounts and the sparse documentation surviving in the Neapolitan archives. Ryder has used these difficult sources with ingenuity, filling in gaps with judicious and well-informed guesses which are always clearly labeled as such. As he reconstructs the administration, section by section, Ryder assesses the extent and impact of Alfonso's reforms and reaches several conclusions which, when taken together, constitute a revision of Neapolitan history. The picture which emerges is of a monarch whose military needs in conquering the Kingdom of Naples and retaining control over the potentially rebellious barony reformed every level of the administration without destroying its ties with the past and with local custom. The author's conclusion that Alfonso replaced the barons in the royal household with his own trusted Spanish vassals but appointed a majority of Italians in almost every other part of the administration, military as well as civil, is amply supported by his evidence. He also convincingly argues that Alfonso's taxation policies developed out of native institutions rather than being wholly alien customs and that Alfonso's economic policies did not damage agricultural development any more than established Neapolitan agrarian practices would have.

The author avoids some serious pitfalls: he does not venture outside his stated limits of institutional history; his anti-Spanish prejudices do not interfere with his judgments of Alfonso and other Spanish administrators; and the conceptual difficulties which one would expect to encounter in a work whose subtitle conjures up visions of Whig history do not arise in the body of the work, though they are present in the introduction and conclusion.

In a work of many strengths, two stand out for their significance and for the difficulty of the achievement. Ryder succeeds in revealing the vitality of both the Italian antecedents and the Spanish reforms of Alfonso's government, and he is able to identify many signatories of royal documents with their careers, offices, and changing status so that names take on body and institutions take on life. This is institutional history at its best: Ryder has provided the institutional framework and peopled it. It will now be the work of Renaissance historians to reassess the lives and writings of Lorenzo Valla, Antonio Beccadelli, and other humanists who produced their most innovative works while they were employed in the Kingdom of Naples under Alfonso the Magnanimous.

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KAROL MALECZYŃSKI. *Bolesław III Krzywousty*. Wrocław: Ossolineum. 1975. Pp. 374. 90 Zł.

The history of this volume is almost as interesting as the book itself. Written prior to 1939, it was being published in Lwów at the moment World War II began. The author salvaged only one fragmentary copy of that edition, and eventually used it as the basis for a rewritten popular biography of Bolesław III the Wrymouth. Published without scholarly apparatus in Cracow in 1946, that book has been for a generation one of the standard treatments of the crucial early eleventh century in Poland. After Karol Maleczyński's death his widow desired that the original scholarly study be published. The present book represents the Lwów text, except for parts of two chapters which had been lost and have been reconstructed from the 1946 Cracow text, with notes provided by a former student, Kazimierz Bobowski. The book is complemented by a brief introduction by Wacław Korta, a sympathetic profile of Maleczyński as scholar and person by Aleksander Gieysztor, and an up-to-date bibliography prepared by Bobowski. The book unfortunately lacks an index and any maps with which to follow the detailed political history of the time.

After nearly forty years this is still an important biography, for it deals with a crucial figure in the history of early Piast Poland. Bolesław was to be the last ruler of a territorially unified Polish state for more than 150 years; and in his lifetime he had to cope with problems of ecclesiastical polity, foreign intervention, dynastic rivalry, a rapidly changing society, and the question of political succession. The way in which he dealt with these problems shaped the Polish scene fully as much as the policies of any of his predecessors or successors. In addition to the intrinsic importance of the topic, the scholarship and literary qualities of this book remain impressive. There will probably not for the foreseeable future be a need for a comprehensive political narrative of Bolesław's reign. In some isolated areas, such as the Merseburg agreement of 1135 and the origins of the political testament of Bolesław, details have been modified and refined; but on balance this book remains a major contribution. Nevertheless, it is not without weaknesses. Its focus is so exclusively upon the foreground of politics and the course of events that the equally important background of society and economy are slighted. The work of contemporary scholars is necessary to provide the needed broader perspective that will complement this still useful book.

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MODERN EUROPE

JACK GOODY *et al.*, editors. *Family and Inheritance: Rural Society in Western Europe, 1200-1800*. (Past and Present Publications.) New York: Cambridge University Press. 1976. Pp. vi, 421. \$19.95.

Inheritance systems, broadly conceived, are strategies for the transfer of property from one generation to another. They include post-mortem settlements, pre-mortem dowries and legacies, provisions for widows, and arrangements for the retirement of the older generation. Demographic behavior, kinship ties, relations between parents and children and among siblings, and residential patterns are all linked to systems of inheritance and modes of land tenure. The ultimate purpose of an inheritance system is to maintain and possibly promote the status of the family and its members. Inheritance systems coalesce in specific historical settings. Reciprocal and mutually reinforcing relations bind inheritance systems to shifting social, economic, political, and legal structures. In most cases the flow of causation runs strongest in one direction. Inheritance customs are essentially final products.

The ten papers in this volume examine the inheritance systems of peasants and nobles in Western Europe in the late medieval and early modern periods from a variety of perspectives. The authors agree that these practices and their implications can only be understood as total strategies which take into account both the development cycle of individual families and the longer view of social classes over time. The analysis must begin with legal formulas and customs and then move on to actual behavior. In theory, impartible inheritance with primogeniture and partible inheritance are opposed. In practice, they often amounted to the same thing.

In preindustrial times, substantial families from humble peasants above the meanest subsistence level to peers of the realm shared two closely related but conflicting aspirations. They wanted to preserve the family name and lineage through the person of a privileged heir, yet felt obliged to provide honorably for as many sons and daughters as possible. What a given family or group of families did depended on the full range of options open to them—the grid of inheritance, to use E. P. Thompson's term. These options increased as market practices expanded, wealth grew more liquid, and employment opportunities more numerous.

In open-field districts, the subdivision of family holdings soon reached the limits of economic viability. Impartible inheritance was customary with younger children receiving cash legacies and dow-

ries. In mixed farming regions with plentiful common land for pasture, the division of the family's arable was less likely to lead to economic ruin, and partible inheritance was the rule.

J. P. Cooper shows that nobles used entails to preserve a central core of properties, but established dowries and legacies for younger children in cash and burdened the entire estate with mortgages to pay them. Behind primogeniture and entail stood a complex web of credit and debt which increased the economic dependence of the nobility on political patronage and court favors.

Yeomen did not have access to the resources of courts and, as Margaret Spufford and Cicely Howell show, in England their modest holdings could not withstand the financial pressures of repeated crop failures or long runs of listless prices. The market frustrated the attempts of the yeomen to do well by all their children and dissolved much of this class long before the eighteenth century. The same thing happened in France.

Hereditary rights to land were of critical importance. The exact legal status of a given acreage expanded or contracted the options of a family. In Germany, Lutz Berkner demonstrates how manorial law artificially restricted the division of peasant tenures and favored stem-family arrangements in one community. Nearby, in a similar geographic and economic setting without legal barriers to division, estates fragmented and residential patterns differed.

Demographic behavior was also linked to land tenure and inheritance systems. Nobles restricted the number of children married with the financial backing of the family, pushed younger sons and daughters into religion in Catholic countries, and even controlled family size in the interests of maintaining status.

The study of inheritance customs leads inevitably to an examination of the entire historical context. The essays in this volume touch upon subjects too numerous to mention here. All are of high quality and point the way to a revitalization of a long neglected topic.

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ANN M. PESCATELLO. *Power and Pawn: The Female in Iberian Families, Societies, and Cultures*. (Contributions in Intercultural and Comparative Studies, number 1.) Westport, Conn.: Greenwood Press, 1976. Pp. xix, 281. \$15.95.

How does one study "females," "miscegenation," "mothers," "families," and "cultures" as well as the complexities of societies as diverse as the Spanish Peninsula, Portuguese Africa, Iberian Asia, and contemporary Latin America in some 250

pages? Indeed the task is an enormous one, but the chore becomes even greater when one tries to explore several centuries of history across four continents, while hoping to develop new multidisciplinary approaches and methods.

Ann Pescatello confronts her goal equipped with an enthusiasm based on her previous research experiences and her "personal affinity for Iberian cultures" (p. 229). What the reader finds lacking is the ability to avoid the often indiscriminate accumulation of historical and anthropological facts and present a more systematic comparative analysis of the role of women and families in various societies and cultures.

The author attempts to show the changing role of women in a vast Iberian world, and how they have been conceived both as wellsprings of power and as pawns in male-dominated societies. In tracing the cultural origins of this dual view Pescatello begins her survey with paleolithic times and in a cursory exploration moves from Mesopotamia to Crete, from Confucian China to Greece, Rome, and the Judeo-Christian world. From this bird's-eye view of several millenia of human development, she ventures the preliminary conclusion that matriarchy and male superiority amalgamated in the Mediterranean as a preview of "Iberia's dichotomous behavioral patterns" (p. 7). The author uses such sweeping generalizations, based on a handful of disparate sources, as a point of departure for her exploration of Iberian societies. She swiftly moves from the Peninsular early Middle Ages to contemporary Spain and Portugal and deduces that, despite similar patterns of behavior and values, differences between groups in both nations depend on social and economic distinctions. Yet there is no attempt to pursue the class analysis any further.

From here the author turns to Asia and looks at India, China, the Philippines, Macao, and Goa, where she sees various levels of amalgamation of Iberian and indigenous values developing through the last four centuries. By contrast, Pescatello states that in Africa there has been little ethnic interaction so that the role of females has remained "relatively traditional." From this she concludes in a rather cryptic way that "for the moment Africa still remains Iberia's black mother" (p. 111).

The next hundred pages are devoted to the Americas, from the great pre-Columbian civilizations to the present. Even though miscegenation and assimilation were common from early colonial times, the author discerns "little contribution toward establishment of a generalizable lifeway for females of contemporary Ibero-America" (p. 130), a point she sets out to demonstrate by reviewing the individual nations. In this exploration Brazil received closer attention than

other hemispheric countries, particularly for the last two or three decades.

Although this book is ambitious, it is not always successful. While the author deals with a fascinating subject heretofore much neglected, she fails to do so convincingly; casting her net too broadly, she is episodic rather than analytic. The specialist will encounter many facts and interpretations with which to quibble, arguments which have not been worked out thoroughly, and terms which demand clearer definition. The style is often discursive and cluttered with unnecessary jargon. Indeed, the reader will find it difficult to disagree with the author's concluding remarks about her own work: "I think that I have made some rather broad and general statements about the nature of human society."

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GEORG DENZLER. *Das Papsttum und der Amtszölibat*. Volume 1, *Die Zeit bis zur Reformation*; volume 2, *Von der Reformation bis in die Gegenwart*. (Päpste und Papsttum, volume 3.) Stuttgart: Anton Hiersemann, 1976. Pp. xii, 180; 182-482. DM 200 the set.

Georg Denzler's *Das Papsttum und der Amtszölibat* constitutes volume 3, parts 1 and 2 of the series *Päpste und Papsttum*, begun in 1971 under the general editorship of Reinhard Elze and others. So far eight volumes have appeared, ranging topically from Teta E. Moehs' *Gregorius I* (1972) to Wolfgang Reinhard's *Papstfinanz und Nepotismus unter Paul I* (1974). As the editors remarked in the preface to volume 1 (Werner Marschall, *Karthago und Rom*, [1971]), the series will consist of biographies of individual popes, particularly those whose pontificates have not received proper scholarly study or about whose pontificates sufficient new information has emerged to require revisions of existing biographies. In addition to these detailed biographical studies of individual popes, the series also proposes to print specialized studies of aspects of papal government and papal influence upon European and world history. The series is international in scope (Moehs' study of Gregory V is in English) and, apparently, endless. The studies printed so far, although they are uneven in quality, will prove extremely helpful to most scholars interested in early, medieval, and modern papal history. Besides Elze himself, the editorial board consists of some of the best European historians of the papacy.

Das Papsttum und der Amtszölibat reflects both the advantages and the disadvantages of such a series. On the one hand, the existence of the series provides an occasion for the publication of a highly

specialized and generally important book. On the other, however, the series has permitted the author to write a narrower, less finished, less thorough book than he should have. The first volume consists of a preface that is much too brief and which completely fails to link Denzler's own work to that of his great predecessors, particularly, for English readers, that of Henry Charles Lea. There follow six chapters, dealing chronologically with attitudes toward clerical celibacy in Scripture, the Fathers, and the early councils, then in the popes, beginning with Damasus I through the pontificate of Julius II. The second volume consists of eight chapters dealing with the period from the pontificate of Leo X to that of Paul VI. The first volume also has an appendix consisting of fifty-one documents. The second volume adds fifty-one more documents, several other appendixes, a bibliography, and an index. The documentary additions are useful, and they bring together important papal and conciliar statements concerning the problem of celibacy. Most of them are briefly noted in the text, but they are printed without commentary.

Although the arrangement of the text and documents is clearly appropriate in chronological terms, the rest of Denzler's work is not. Each pope is discussed in turn, the discussion often consisting chiefly of rather well-known aspects of the pontificate. The relentless homogeneity of Denzler's format requires the same treatment of each pope mentioned, even if, as in the case of Paul II, all that Denzler has to say is, "Of Paul II it is well known that he had a daughter." The format, in short, does not lend itself to a thematic discussion of clerical celibacy, and although Denzler does not claim to have written a complete history of clerical celibacy, neither has he written a satisfactory history of the papacy and clerical celibacy. These volumes appear more like the materials for a history of the papacy and clerical celibacy than a history itself, even by Denzler's definition.

Although it is very handsomely presented, the book is not free of errors. The emperor Theodosius I died in 395, not 387 (1:12); *Digamist* (1:21) is satisfactory Greek, but not common German usage. In general, the period since the sixteenth century is more extensively and better treated than the period before, and Denzler effectively deals with the current place of clerical celibacy in the counsels of the modern and the contemporary papacy. Within the author's definition of the book's limits, then, the study is useful and adequate, but the social historian will find little here of interest. The problem of clerical celibacy touches many different aspects of medieval life and constitutes an important part of medieval cultural and social history, from aspects of attitudes toward, and the status of, women, to the nature of

the clerical order. Denzler's chapter 4 of volume 1, which deals with the period between 1112 and 1144, when these questions were particularly acute, does not satisfactorily answer most of the questions modern medieval historians would ask. Thus, although this is a handsome, carefully researched, and useful book, it is far from being the final word on a vexing and persistent historical problem.

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KENNETH C. SCHELLHASE. *Tacitus in Renaissance Political Thought*. Chicago: University of Chicago Press. 1976. Pp. xiii, 270. \$16.00.

Tacitus can be cited against or in favor of monarchy, as a stern moral judge of political behavior or as a "machievellian" adviser, as a historian who told the truth or as a purveyor of lies. He has been different things to different men, and even to the same man at different stages of his career.

Accordingly, Kenneth C. Schellhase has not been able to reduce the impact of Tacitus to a simple pattern. Instead, he has analyzed his influence on individual scholars, in chapters under titles such as "Tacitus in the Political Thought of Fifteenth-Century Italian Humanists" and "Tacitus in Reformation Political Thought." The principal subject of the first chapter is Leonardo Bruni, "who first came to see the importance of Tacitus as a source of political and historical ideas" (p. 20). In chapter two the principal subjects are Conrad Celtis and Ulrich von Hutten; in chapter three, Aventinus and Beatus Rhenanus; in chapter four, Machiavelli, Alciato, and Guicciardini; in chapter five, Curione, Patrizi, Bodin, Lipsius, and Botero; in chapter six, Montaigne, Lipsius again, Davanzati, Ammirato, and Boccalini; in chapter seven, Strada, Francis Bacon, and Vico.

Before discussing the impact of Tacitus in each case, the author gives a brief sketch of the historical situation and of the development of the particular scholar's ideas. This is based on a close comparison of critical passages to corresponding passages in Tacitus, as well as on the interpretations of modern commentators, which are succinctly summarized. The result is a work of erudition that should be of great use to graduate students and their teachers.

In general, Schellhase finds that Tacitus was used for political purposes (whether republican or monarchical) in periods of crisis, for example by Bruni in the defense of republicanism in the beginning of the fifteenth century (here the author follows the interpretation of Hans Baron). After the advent of the Medici, the Florentines "almost forgot about Tacitus altogether" (p. 25), until a new

crisis at the end of the century inspired Machiavelli to make political use of Tacitus again. The section on Machiavelli contains, in my opinion, some of the most controversial and interesting material of the whole book.

The quiet periods between crises tended to produce nonpolitical scholarship, but what the author has to say about the work of scholars like Beatus Rhenanus or Lipsius is not less interesting or less valuable. In the last chapter, on the seventeenth century, the author sustains his argument by pointing to the contrast between Tacitan scholarship in England and on the Continent. Tacitus was an important resource for Englishmen engaged in the political struggles of the first four decades of the century, but after 1660 he "became as useless for politics in England as he had become on the Continent after 1613" (p. 165). The fact that thirty-five Latin and English editions of Tacitus were brought out in England in the years before 1800 suggests, however, that he was read in quieter periods as much or more than in periods of political crisis.

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MANUEL FERNANDEZ ALVAREZ. *Charles V: Elected Emperor and Hereditary Ruler*. Translated by J. A. LALAGUNA. (Men in Office.) London: Thames and Hudson; distributed by Transatlantic Arts, Levittown, N.Y. 1975. Pp. 220. \$15.00.

WALTHER HUBATSCH. *Frederick the Great of Prussia: Absolutism and Administration*. Translated by PATRICK DORAN. (Men in Office.) London: Thames and Hudson; distributed by Transatlantic Arts, Levittown, N.Y. 1975. Pp. 303. \$15.00.

PETER PIERSON. *Philip II of Spain*. (Men in Office.) London: Thames and Hudson; distributed by Transatlantic Arts, Levittown, N.Y. 1975. Pp. 240. \$15.00.

Biography is not very highly regarded by many professional historians. Yet the genre is still triumphantly with us. The educated reading public likes it, college students demand it, and even the most convinced followers of the *Annales* school have been known to murmur that it would be good to have a modern biography of so-and-so. Most historians will therefore welcome the new series "Men in Office," edited by Ragnhild Hatton, for it is designed to provide biographies of leading statesmen which are scholarly, up-to-date, and readable, and which include the fine illustrations for which this firm of publishers is justly famous.

The first three volumes to be published generally fulfill these expectations. They also provide fascinating contrasts, both in the personalities of their subjects and in the historiographical approach and

method of their respective authors. Manuel Fernandez Alvarez is an old-fashioned historian. He tells us nothing about demography, wages, or prices, and little about finance. His discussion of the logistic problems of Charles V's empire is superficial. But his Charles and the personalities with whom the emperor had to deal are believable men and women of flesh and blood, with clear-cut political, religious, or plain self-serving motives. Fernandez has read all the many published volumes of the emperor's and his ministers' correspondence, and he has delved deeper than most of Charles' biographers into the almost limitless riches of unpublished sources in the Spanish, Italian, German, Dutch, French, and English archives.

The resulting portrait has no surprises. Charles was a man whose motivation was essentially religious; he was also a highly competent and, in the end, very experienced politician who tried to deal with the problems he had to face coolly and systematically. His reign cannot be written off as the complete failure it is so often represented to have been.

Neither his contemporaries nor later historians have found the personality of Philip II as attractive as that of his father. It has, however, aroused much greater passions; for, much more uncompromisingly than the emperor, Philip seemed to be involved both in the defense of the Catholic Church and in the attack both on all forms of Protestantism and on the national existence of England, France, and the Netherlands. Philip's theologians never attempted to find compromises with the Lutherans, as Charles V's had done at Augsburg and Regensburg, and the genuine internationalism of his father's empire seemed to have narrowed into a crude Spanish imperialism. Not altogether surprisingly, there has so far been no major balanced biography of Philip II such as Brandi's of Charles V. Nearly all previous attempts have been either indictments or apologies.

Peter Pierson has broken out of this pattern. He has given us by far the most balanced biography of Philip II we have had so far and, in comparison with the other volumes of this series, also the most comprehensive. Pierson knows all about the *Annales* school, as well as the psychological speculations of the late Gregorio Marañón. At the same time he has mastered the more traditional sources of Philip's official and private correspondence. His judgments are judicious and careful. Here is a good example: "Philip's refusal to follow up his victory at St. Quentin . . . with a march on Paris has been much criticised, then and since. But he was short of funds, autumn was coming on, and before him lay a formidable array of fortified towns and Guise's army, back from Italy. Above all,

Philip did not want war, but peace; his ideals were religion and justice, for which he would fight if he had to. At some point, he had ceased wanting to be a warrior. He was no longer the youth who in 1552 had begged to be permitted to fight at his father's side in Germany, and who in 1555 had wanted to lead an army. Was it the carnage of St. Quentin, or the embarrassing cost of war to his treasury and thus to the prosperity of his subjects, that sapped his enthusiasm for war? Later in his reign, at war on all fronts, he frequently complained in marginal notes about the cost of war, but not about the human suffering it caused. But statesmen seldom speak much of suffering, whatever they feel. What little Philip did say suggests that it weighed on his soul, not because of his regard for the judgment of mankind, but because of the judgment of God" (p. 34).

At first sight, Frederick the Great seems to belong to a totally different world from the two sixteenth-century Habsburgs: the eighteenth-century ruler of a small northern country who developed an administrative apparatus and a military machine to such a formidable degree of efficiency that he could successfully take on all the great military powers of his day with the sometimes dubious help of only Great Britain. Yet a closer look shows Frederick II still as much a Renaissance prince as a modern head of state. This friend of the philosophes, *littérateur*, and amateur musician preferred to live in a small palace, Sans Souci, which resembled a Medici country house much more than the huge pile of Versailles. Like a Renaissance despot he interfered with the administration of justice and had judges and civil servants who displeased him imprisoned on his mere word. More even than Philip II he regarded his ministers as no more than his executive agents, although in practice many of them did exercise considerable initiative.

Walther Hubatsch has concentrated on Frederick's personality and on his work in the government of Prussia. He has largely left out his foreign policy and his campaigns, arguing that these have been sufficiently treated by other historians. The result does not always make easy reading, but it does give a comprehensive account of how the Prussian administration really functioned and of the king's economic policies. These were indeed impressive and they were also the most "modern" aspects of the reign.

All this is excellently done. On Frederick himself, the author is respectful but by no means uncritical. What he takes for granted, however, and thereby justifies, are the underlying aims of this monarchy, and these were essentially military and aggressive. It is certainly the biographer's task to lay bare the motivations of his subject and to do his best to treat them sympathetically. But that is

not the same as simply taking them for granted, just as no modern biographer can accept at face value Philip II's view of his championship of the Catholic Church. For the rest, this is a most useful addition to the still sparse modern literature in English on Frederick the Great.

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GEORGES LIVET. *L'équilibre européen de la fin du XV^e à la fin du XVIII^e siècle*. (L'historien, number 28.) Paris: Presses Universitaires de France. 1976. Pp. 231.

Several years ago Gordon Craig called for a new approach to diplomatic history. He said it should focus on the ideas and institutions which helped to mold policies and on the relationship between ideological, socioeconomic, or institutional structures and personality. ("Political History," *Daedalus*, 100 [1971]: 334-35). Georges Livet has written such a study. Drawing on sources ranging from ancient to modern times, from literature to the social sciences, from Fénelon to Marshall McLuhan, Livet forces us to rethink not only how early modern diplomacy worked but even the very meaning of diplomatic history. his sweeping approach to history is reminiscent of William McNeill's.

After discussing the meanings of "equilibrium" in physics, religion, etc., Livet outlines European international relations from the end of the Middle Ages until the French Revolution in his "Profils" section. Although the minimal academic apparatus indicates he is addressing students and general readers, the rapid sketch of events will have little value for novices. For example, on page 44 the author names five treaties and meetings in eight lines with almost no explanation. Only scholars well acquainted with the period will understand the significance of Livet's interpretations and how they fit into the broader pattern he is developing. Furthermore, specialists will often question his statements about why decisions were made.

In part two Livet examines the "Structures" of the European equilibrium—models, mechanisms (alliances, mediation, and war), forces (economics and public opinion), and myths (security, peace, and law). Livet's discussion is occasionally unclear because of his tortuous prose, but more importantly because of his subject's imprecision. His European equilibrium is not clearly defined because it was never static enough for any definition to fit. Ultimately, Livet thinks that the equilibrium constantly changed, re-establishing itself after each perturbation only to be unbalanced once again. The equilibrium operated like an Adam

Smithian invisible hand rather than as the result of rulers' conscious desires.

Quantifiers and theoreticians of international relations will be disappointed. Livet's approach is humanistic. Diplomatic historians, early modernists, and scholars interested in contemporary French historical writing will be fascinated and frustrated by this very worthwhile study.

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PAUL J. KORSHIN, editor. *The Widening Circle: Essays on the Circulation of Literature in Eighteenth-Century Europe*. (Haney Foundation Series, number 20.) Philadelphia: University of Pennsylvania Press. 1976. Pp. 204. \$15.00.

The vast subject of the history of the book trade includes the study of authors, publishers, readers, and the contents and production of the books themselves. The four essays under review exemplify how the field is accommodating diverse scholarly interests and is taking new directions.

The first essay by Paul J. Korshin introduces the other three, which were originally delivered at the 1974 meeting of the American Society for Eighteenth-Century Studies. Korshin surveys recent research and techniques in the investigation of literacy, book subscription lists, sales catalogues, libraries, and clandestine publications. The valuable bibliographical references cover mainly the English book trade.

Roy M. Wiles draws evidence largely from local English newspapers, especially their advertisements, to discuss a neglected subject—the amount of reading and the types of reading matter available in eighteenth-century England outside of London. Without being able to measure the exact size of the reading public, he concludes from a great many specific examples that "increasingly in successive decades multitudes of English people developed and enjoyed a relish in reading" (p. 112). Wiles' essay would have been enhanced by more explanation of the causes and effects of this growing appetite for reading and by comparison with other areas of Europe.

The essay by Bernhard Fabian concerns various aspects of the translating, reprinting, selling, reading, reviewing, and collecting of English books in eighteenth-century Germany. We learn, among many other things, that French books remained more sought after than English books, but that after 1750 demand increased for English books. By 1800 the German reading public was well acquainted with English literature and culture. A mine of useful information, this essay is more a series of observations and tentative conclusions than a synthesis.

Robert Darnton's long essay is a fascinating account of Bruzard de Mauvelain, an eighteenth-century French clandestine bookseller of obscene and radical literature at Troyes. First, Mauvelain's dubious character and shady dealings are vividly described. We follow his negotiations in the 1780s with a supplier, the *Société typographique de Neuchâtel*, Switzerland, and the smuggling operation used to provide him with books. Then, the books he ordered are examined. They reveal little demand for works by the philosophes except for those by Helvétius, La Mettrie, and especially d'Holbach, more demand for pornography and impiety by hack writers, and most demand for material on politics and other contemporary affairs. Darnton thinks further research will indicate that Mauvelain was typical and important. Other obscure booksellers throughout France sold tracts, scandal sheets, and libels against officials and courtiers similar to the ones he requested. Such works were extremely influential in unintentionally undermining the Old Regime. This essay is a painstaking and pioneering case study of the underground book trade on the eve of the French Revolution.

In sum, these four learned essays provide new perspectives to historians, men of letters, librarians, and bibliographers and help to break down artificial barriers separating them.

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DANIELE MENOZZI. *"Philosophes" e "Chrètiens éclairés": Politica e religione nella collaborazione di G. H. Mirabeau e A. A. Lamourette (1774-1794)*. (Istituto per le Scienze religiose di Bologna. Testi e ricerche di Scienze religiose, number 11.) Brescia: Paideia Editrice, 1976. Pp. 425. L. 8,000.

ALFONSO PRANDI. *Cristianesimo offeso e difeso: Deismo e apologetica cristiana nel secondo settecento*. Bologna: Il Mulino, 1975. Pp. 510. L. 10,000.

CLAUDIO DONATI. *Ecclesiastici e laici nel trentino del settecento (1748-1763)*. (Studi di storia moderna e contemporanea, number 5.) Rome: Istituto storico italiano per l'età moderna e contemporanea, 1975. Pp. 338. L. 10,000.

The past few decades have seen a new flowering of interest in the religious history of Western Europe in the eighteenth century. This increased scholarly activity is due in part to the innovative methodologies developed by historians who have applied the techniques of sociology and other social sciences to religious studies. In this realm the contributions of French specialists have been pre-eminent.

Yet these three recent Italian books testify to the international dimension of the renaissance in reli-

gious history. Viewed collectively, the three works are particularly interesting for their illustration of the continuing cross-fertilization of Franco-Italian intellectual life, a process so apparent in eighteenth-century religious thought and evident today in the awareness of recent French scholarship in which the "new" history is gradually being refined.

Claudio Donati's ecclesiastical history of the region of Trentino is a competent regional study, displaying the strengths and handicaps of that approach. Although limited in perspective and chronological coverage, this book offers some valuable findings. Trentino was a unique area of cultural passage between Austria and Italy because of its special historical and geographical position, and the salutary influence of Austria in Trentine religious affairs constitutes a secondary theme of the study. Prior to the arrival of the reforming bishop, Leopoldo Firmian, son of an illustrious Tyrolean family, the clergy of Trentino were plagued with internal overpopulation, predominance of secular values, and inefficient administration. Within the city of Trent itself clergy constituted over five percent of the population.

The unusual character of the Trentine parochial structure is of special interest: ninety percent of the parish clergy were indigenous, with at least half of these drawn from the noble class. Lacking curial benefices or ecclesiastical patrimonies, many priests were ordained somewhat irregularly *sub titulo capellaniae*, as chaplains in the service of lay notables. Far from mainly spiritual, their clerical role was thus degraded to that of a servant employed in the household of the notable as overseer of peasants or as administrator of the estates of the lay patron. Donati stresses the significance of Firmian's pastoral visits of 1749-51 in tightening parochial discipline. His conclusions here should be compared to those of Louis Pérouas, Dominique Julia, and Berthelot du Chesnay for France. In a similar vein, Firmian's efforts to reform the popular religion of his diocese, heavily baroque in its long processions, elaborate ceremonies, and emphasis on the cult of the Virgin and the saints, provides a striking point of comparison to the recent studies by Michel Vovelle of baroque religion in Provence (cf. my review of his *Piété baroque et déchristianisation*, *AHR*, 81 [1976]: 1125-26).

Daniele Menozzi's study of Mirabeau and Lamourette, their careers, religious and political convictions, friendship, and collaboration in the early years of the Revolution is lively and rewarding. To a certain degree, the two shared a vision of a pure form of Catholicism typified by the exemplary lives of some parish clergy. This pure religion contrasted with the corrupted form that had stemmed from the close union of the Church with

secular and governmental interests. Despite his critical stance toward Catholicism and his warning of the dangers arising from the state's use of religion as an instrument of political control, Mirabeau believed that Christianity had real potential in the service of an enlightened cause; yet its potential could not be realized without the confiscation of Church property, the gradual elimination of monasteries, and the independence of the Gallican Church from Rome.

As a leading exponent of enlightened Catholicism, Lamourette glorified the image of the rural parish where priest and parishioner shared poverty and sacrifice in the tradition of the early Church. If Lamourette was once ardent in his defense of some institutions of the Church against the attacks of the philosophes (as with monasticism), he came in time to moderate his views of the philosophes in works like the *Délices de la religion*, in which he praised Rousseau for his goal of making men happy. For him the Christian religion was to play a major role in determining the criteria of a new society, and, at the onset of the Revolution, he believed that even in a society based on popular sovereignty a Rousseauist social contract would be operative.

Having met at the salon of Madame de Sillery, Mirabeau and Lamourette became close friends and in 1789-90 collaborated on political and religious matters. Initially wary both of violence and its advocates and of a militia which might control popular uprisings, they supported the abolition of the tithe and the articles of religious freedom. Failing to produce a political theology for the revolutionary state, they concentrated on the process of reforming the Church. Lamourette emerged as an advocate of the Church as a "*démocratie chrétienne*," which would guard the ideals of liberty and spiritual happiness promised by God. Since its excessive wealth and temporal power were the sources of its weakness, the new Church must accept the loss of its property and temporal influence and expand its social role.

Alfonso Prandi's study focuses on the reaction in France and Italy to the book *Examen critique des apologistes chrétiens*, which appeared in 1766 attributed to Nicolas Fréret (1688-1749), permanent secretary of the *Académie des Inscriptions et Belles Lettres* of Paris. This book became the center of a literary polemic because the author presented himself not as a sarcastic commentator but as a friend of virtue and healthy morality. The French theologian Nicole Bergier, author of a previous book in reply to Rousseau, was the first to reawaken interest in Fréret in refuting the *Examen*.

Italian apologists for Christianity found Bergier's refutation unconvincing and wrote their own critiques after academic discussions at the univer-

sities of Padua and Pisa. The bulk of Prandi's study is devoted to these Italians: Antonino Valsecchio, professor of theology at Padua; Vincenzo Fassini, whose erudite reply in Latin closely followed Valsecchio's thought; and, the ablest of the apologists, Nicola Spedalieri, a theologian who expanded on Valsecchio's ideas in offering a closely reasoned response to Fréret which took particular exception to his logic in interpreting the Gospels.

Certainly these studies are a testimony to the vitality of Italian religious historiography. The specialist will find in Donati's monograph an illuminating and detailed account of much value for comparative purposes; Menozzi's work provides valuable insights into the religious policies in the early years of the Revolution; and Prandi's account, although a more conventional literary history, underscores the seminal quality of French religious thought in the eighteenth century.

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JOSÉ ANTONIO FERRER BENIMELI. *Los archivos secretos vaticanos y la masonería: Motivos políticos de una condena pontificia*. Caracas: Universidad Católica "Andrés Bello," Instituto de Investigaciones Históricas. 1976. Pp. 870.

Scholars are sure to welcome this new book by José Antonio Ferrer Benimeli, who has been studying for years the evasive role of masonry in eighteenth- and nineteenth-century political life. This volume—divided into seven parts with several chapters in each—traces both the falsified and the true images of the masons. The main sources of his research are the documents in the Vatican, but the author also makes wide use of other archival material. Throughout eighteenth-century Europe groups of persons dissatisfied with the Church created a new religion based on philanthropic ideas. The new code of ethics was not only forbidden by the Church through rules issued by Clement XII (1738) and Benedict XIV (1751), but also by the state: Holland in 1736, Geneva in 1737; France, Germany, and Sweden in 1738; Vienna, Naples, Spain, Madeira, and Prussia in 1743. Throughout the *siècle des lumières* prohibitions were issued against the newly founded groups of philosophes, which met clandestinely and whose members were supposed to overturn the political balance of every country.

European governments accused masons of heresy and of subverting the security of the state. Both masons and masonry were harshly persecuted, in universities, academies, and intellectual centers. Ferrer Benimeli studies in detail various trials from 1740 to 1751. For example, Dr. Crudeli in

Florence; Baptista Richart and Lamberto Bolanger in Lisbon; the voluntary denunciations of Le Roy and Clawes in Spain; and the famous, although now little known, case of the Frenchman Tournor in Madrid (1757). Among the most notorious trials are those of the Count of Cagliostro and Domenic Fontana (Italy) and Burdales and Lausel in Mexico. North America does not escape his scrutiny; he analyzes the trials here from 1791 to 1794.

Finally, the author studies the religious attitude, rites, and philosophical background of masonry. He also deals at length with the membership of priests in the various lodges. The last chapter of the book gives a general catalog of the members of the clergy who were masons. The list includes few Spaniards, whereas Italians and Frenchmen abound, a fact that shows the very traditional attitude of the Peninsular Church.

Ferrer Benimeli is to be highly commended on his research; however, some questions arise as to the role of masonry in the development of egalitarian ideas in the eighteenth century. He is absolutely right in stressing that masonry did not flourish in Spain at this time except for a few isolated cases. I disagree, however, with his conclusion that masonry is not linked to secret societies (for example, *carbonari* and *comuneros*). My own research in the field has shown that since the early nineteenth century there has been a schism among masonic groups, and the more politically and socially minded have created their own sects. Masonry's legacy to progressive and radical groups is manifold, but its use of secret organization and oaths has been most important in providing a model for clandestine political movements. It is not coincidental that Michael Bakunin used the masonic organization to create his secret alliance in 1864, and he confessed that he had belonged to a lodge in Italy and had been a mason from 1845 to 1848.

I still believe that the relationship between masonry and secret societies cannot be explained in a mere digression, as Ferrer Benimeli tries to do. The scrutiny of archives has convinced me to the contrary. The still-Christian formulas and rituals of the masons point to the fact that they reserved for the elite the right to decide political issues. The *comuneros* and *carbonarios* adapted these rituals to a more politicized and democratic movement. Although masons hoped to liberalize the Church and favored philanthropy, they left little activity for the political majority—the *pueblo*, an ever-changing and ill-defined entity—which the secret societies, and first political parties, hoped to win. Masonry thus remained an elitist body of learned men, afraid of the masses.

These last comments notwithstanding, Ferrer

Benimeli has written by far the best book in Spanish on eighteenth-century masonry. It will surely emend many of the standard prejudices of ill-informed historians.

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PAUL BAIROCH. *Commerce extérieur et développement économique de l'Europe au XIX^e siècle*. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques, Civilisations et Sociétés, number 53.) Paris: Mouton. 1976. Pp. 355. 92 fr.

The content of this ambitious study is even broader than the title indicates. It is, in effect, a general economic history of Europe in the nineteenth century with special emphasis on foreign commerce and rates of economic growth. Readers untrained in statistical methods should not be deterred by the book's basically quantitative orientation; they should, however, be warned against accepting its conclusions as definitive or even highly probable.

The volume is divided into two parts. The first, a "global analysis" of European commerce and economic development, contains seven chapters: "the place of the nineteenth century and of Europe in the process of development" (a general introduction); demography, technology, transportation, and precious metals; a brief survey of commercial policy from 1815 to 1914; the "evolution of European foreign trade," containing the basic statistical tables on that subject; foreign investment and international migration; "industrialization"; and "economic growth." The two last-mentioned present important, if debatable, original indices of industrial production and tables of gross national product, respectively. The second part, comprised of case studies, opens with two brief chapters on the general economic situation around 1860 (Great Britain is pictured as a "super dominant economic power") and contemporary and later thought about the merits of free trade. Separate chapters are devoted to commerce and economic development in Great Britain and France; Germany, Italy, and the other "large countries" (Austria-Hungary, Spain, and Russia) are treated more briefly; and, refreshingly for a book of this nature, a separate chapter deals with the smaller countries (Denmark, Portugal, Belgium, Switzerland, and Sweden). This part ends with a chapter of conclusions from the case studies. A general conclusion, a statistical and methodological appendix, and a bibliography complete the volume.

Throughout his analysis the author divides the nineteenth century (up to 1914) into three somewhat uneven segments: a period of protectionism in commercial (especially tariff) policy up to about

1860; a period of free trade from 1860 to 1880 or 1890 (the period differs for different countries); and a renewed period of protectionism until the outbreak of World War I. The two principal conclusions that emerge from the study are that 1.) the growth of incomes (or production) was more often a stimulus for the growth of foreign trade, rather than the converse, and 2.) the growth of both incomes and foreign trade was more rapid in the periods of protectionism than in that of free trade. Unfortunately, in my opinion, neither conclusion will withstand critical scrutiny—which does not imply that the opposite was the case, merely that the author's evidence is not sufficiently robust.

The second conclusion is the more vulnerable. Paul Bairoch, a professor of economic history at the University of Geneva, has valiantly attempted to bring some coherence and consistency to the scattered and contradictory foreign trade data of the nineteenth century; and his estimates of industrial production and national income for Europe as a whole and for individual countries (including Bulgaria!) are stunningly bold, to say the least. The underlying data, however, are simply too meager to support the kind of detailed calculations he has made. Moreover, his methods are hazy, if not suspect. The brevity of a review precludes detailed criticism, but it would be impossible for another qualified scholar to reconstruct Bairoch's estimates on the basis of the data and explanations presented. For example, on page 84 he footnotes one important set of estimates with the phrase, "*Nos estimations d'après diverses sources.*" Such references abound.

Bairoch is cheerfully candid about the tentative nature of his estimates and the large margins of possible (or probable) error inhering in them, but he does not indicate (except in the appendix) what those margins are or how (not even in the appendix) he arrived at them. Having cautioned readers about the weakness of his estimates, he confidently draws strong conclusions from them as if they were entirely reliable. Additionally, there are factual as well as interpretive errors: Sweden, for example, is included among the net exporters of capital in 1913, when in fact it was a substantial importer. The headings of many tables, along with their contents, are misleading or incorrect, and in some cases the estimates are clearly biased.

These technical criticisms notwithstanding, I believe that Bairoch has performed a valuable service in providing us with his estimates, as daring and inaccurate as they may be. Now, at least, other scholars can refine them through criticism, and eventually provide a firm quantitative basis for the economic history of nineteenth-century Europe.

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REINHARD RÜRUP. *Emanzipation und Antisemitismus: Studien zur "Judenfrage" der bürgerlichen Gesellschaft.* (Kritische Studien zur Geschichtswissenschaft, number 15). Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 208.

JEHUDA REINHARZ. *Fatherland or Promised Land: The Dilemma of the German Jew, 1893-1914.* Ann Arbor: University of Michigan Press. 1975. Pp. x, 328. \$12.95.

SIDNEY M. BOLKOSKY. *The Distorted Image: German Jewish Perceptions of Germans and Germany, 1918-1935.* New York: Elsevier Scientific Publishing Company. 1975. Pp. xii, 247. \$10.95.

LUCY S. DAWIDOWICZ, editor. *A Holocaust Reader.* (Library of Jewish Studies.) New York: Behrman House. 1976. Pp. xiv, 397. \$12.50.

FLORENCE R. MIALE and MICHAEL SELZER. *The Nuremberg Mind: The Psychology of the Nazi Leaders.* With an introduction and Rorschach records by GUSTAVE M. GILBERT. New York: Quadrangle/New York Times Book. 1976. Pp. xiv, 302. \$10.95.

DAVID VITAL. *The Origins of Zionism.* London: Oxford University Press. 1975. Pp. xvi, 396. \$22.00.

Contemporary Jewish life is dominated by three experiences that are almost unique in the history of modern nations. One was the emancipation of Jewry from a semi-medieval ghetto existence to an integral role in the intellectual and economic activity of the most important centers of the Western world. Another was the physical annihilation of the European Jews during the Nazi regime. The third was the resurrection of a language and the re-establishment of a state after almost two thousand years of dispersion. All three of these events transpired in the course of only about a hundred years within most recent memory, and all three were political occurrences in the context of major upheavals and convulsions.

So great has been the impact of that three-fold development on the Jewish community that almost any description of its present state would have to deal with those three topics. To the extent that current writing is focused on de-ghettoization, destruction, or Israel, it is a literature of political science and political history. The six books at hand are a small part of that continuing effort.

Three of the titles will augment a growing shelf of books about Jews in Germany before 1933. Why that preoccupation, to this very day, with Germany's prewar Jews? Was detailed information not already plentiful about them ten years ago, or thirty? Clearly, the problem is one of interpretation. Whatever may have been written before the Holocaust was based on assumptions of gradual changes and trends, not sudden obliteration. Thus, Felix Theilhaber's classic demographic

work of 1911 prophesied a decline of German Jewry because of its falling birthrates, increasing intermarriages, and progressive assimilation. Those were the worries then. More recently, one started out to explain why it is that the Holocaust was not foreseen, and what kind of phenomena, particularly in Germany itself, gave rise to it. That is the issue now.

Reinhard Rürup's collection of essays on emancipation and anti-Semitism has previously appeared as separate articles. Together they are an attempt to come to grips with two opposing and contradictory forces, one drawing the Jews into the German mainstream, the other expelling them from it. The essence of Rürup's thesis is that both of these movements stemmed from larger concerns and that both were aimed at wider ends. In brief, there was no "Jewish question" before the end of the eighteenth century, only a policy toward the Jews. The Jewish situation became a problem, however, when a static society was transformed into a dynamic-competitive one. Emancipation was thus not a product of philo-Semitism, but an outgrowth of the recognition that the existing state of affairs could not continue. Jewish liberty was granted step by step, sometimes in anticipation of Jewish services, occasionally as a reward for them. In the main, the Jewish breakthrough was confined to relatively narrow elites, and even that limited success could be maintained only with political and economic stability. In Rürup's view, conditions were not stable enough. The year 1873 marks the beginning of the economic cycles of prosperity and depression, and shortly thereafter the anti-Semitic movement surfaced in the political arena. The "Jewish question" which had heretofore been concerned with the emancipation of the Jew now became a demand for an emancipation *from* the Jew.

Neat, almost too neat. How close to an apologia such an explanation can come. Rürup does remind us that nowhere did both emancipation and anti-Semitism generate so much discussion as in Germany. In no country of Western Europe were these agonies so prolonged. The emancipators thought of themselves as reformers in an educational and philosophical sense. The anti-Semites also offered ideas and ideals, but theirs was a "theory deficit," for they chose one factor as the root cause of all of Germany's ills. They fixed their tunnel vision on the Jew. The anti-Semites were typical Manicheans, for they believed that good is achieved when evil is destroyed. They did not dream, Rürup says, of the consequences of such beliefs.

Neither did the Jews. The Jewish reaction to the anti-Jewish threat is the subject of two new volumes, one by Jehuda Reinharz, the other by Sidney Bolkosky. Reinharz covers 1893-1914. Bolkosky follows with 1918-35. The two works might

be deemed complementary, but they are very different books.

Reinharz is meticulous; he has been to all the archives, he has read all the periodicals, including some of the most obscure. There is probably more detail in this book than most people would want, yet it is detail not only about Jewish organizations and personalities, but also their ideas. Reinharz is particularly interested in the way in which Jewish writers and spokesmen defined their affiliation to Germany and to Jewry. It is that problem which is reflected in his title—fatherland or promised land. He quotes at length from men like Franz Oppenheimer who constructed a distinction between a consciousness of descent which Jews shared with each other and a sense of belonging, binding them to the Germans. German Jews, according to Oppenheimer, were nationally and culturally German. Eastern Jews were nationally and culturally Jews. Much later, German Jewry's principal representative body, the Central Association, was to reiterate that Jewry was a community of descent, but now it added that Jews were also a community of fate.

If the Zionists wanted Jews to have something more than a common fate, they were clearly not the predominant voice of German Jewish society. In that Reinharz gives them equal space in his book, he defies the proportions established by history itself. That is not merely a matter of numbers: the stance of the Zionists in Germany is not to be confused with the position of Zionists elsewhere. Oppenheimer, for example, was a Zionist, yet his Germany was holy to him. In the German Jewish community, non-Zionists and Zionists could agree that Palestine was for Eastern Jews. There is a table in one of the chapters on Zionism which shows that from 1920 to 1932, the Jews of Germany, numbering more than a half million, migrated to the promised land at a rate of less than a hundred a year.

For Sidney Bolkosky, the attitudinal pattern of the German Jews was fraught with lethal consequences. The Jewish perception of Germany was a distorted image, the German Jews were ensnared in an illusion. Page after page of Bolkosky's book is devoted to the mode in which Jews built and clung to the myth of security. They admired Goethe and Schiller, and loved Lessing for his "Nathan the Wise." They attempted to show that there was a linguistic bond between Germany and Jewry in the similarity of Yiddish and Middle High German. Even religious differences were to be bridged—the apostle Paul was a Jew and his ethics were common to Christian and Jew alike. Much in Bolkosky's analysis is literary discussion and his *dramatis personae* are as apt to be poets and literary figures as functionaries and politicians. He is after the molders of the myth and dissects them

mercilessly. He sees a fatal weakness in Jewish attachments to German culture and attacks distinctions between 1932 and 1933 as fatuous. Among postwar writers he singles out for a touch of criticism the historian Peter Gay who, he feels, is an echo of an earlier Jewish generation which embraced Germany's enlightenment unto death.

The Holocaust is a persisting subject in Jewish thought and it will remain so for decades to come. One of the latest additions to the literature of Jewry's destruction is Lucy Dawidowicz's *Holocaust Reader*. It is a compilation of laws, diaries, speeches, conference minutes, and poems that serves as a source volume for her previously published *The War Against the Jews*. That book in turn is the "companion text" for the reader. *The War* reads like a long term paper, divided into two unconnected parts, one sketchily describing the Germans, the other dwelling on the Polish Jews. In that treatise, she resurrected some discarded notions (particularly the supposition that Hitler thought of the "final solution" when he wrote *Mein Kampf*) and provided a nostalgic account of Jewish life in the ghettos of Poland, with particular regard for the suffering of observant Jews. *The War Against the Jews* was not a significant contribution to knowledge, and neither is the *Holocaust Reader*. Virtually all of the German documents in the reader appeared in English translation years ago; they are all very well known. Some of the Jewish items, such as the communiques of the Jewish Fighting Organization in Warsaw, are also old news. Much of the remaining material lacks either novelty or importance. Dawidowicz does include selections from the diary of Adam Czerniakow, chairman of the Jewish Council in the Warsaw ghetto. The passages were translated into English from the Hebrew "version" by Zeva Shapiro and "edited in accordance with the Polish original" by Szymon Dawidowicz. They include excerpts, but there are no ellipses to mark omitted sections. At the end of the reader, a statistical recapitulation of the Jewish dead (taken from *The War Against the Jews*) quite obviously rests on misconceptions of wartime geographic boundaries with the result that for several regions the Jewish victims are counted twice.

If Dawidowicz has little to offer to the serious student of the Holocaust, the description by Florence Miale and Michael Selzer of the "Nuremberg mind" is for all practical purposes a comic strip. In a word, the authors insist that the Nazi leadership as represented by defendants at the first Nuremberg trial was a group of psychotics. The evidentiary material for this contention consists entirely of a Rorschach test administered to the accused in their cells. There, in the shadow of the gallows, they were asked what they saw in the inkblots. The responses, even under prodding,

were somewhat sparse and, at times, pedestrian. No matter. Frank was "irrational," Göring and Fritzsche "psychopathic," Speer "buglike," Kaltenbrunner "paranoid," Funk a "jackal" and a "man of great violence and brutality." Keitel gets by with "psychotic traits," and von Neurath suffered only from a feeling of being "inferior and therefore undernourished, undernourished and therefore inferior." Seyss-Inquart on the other hand was afflicted with an underlying "ferocious violence," even if "in a psychopathic way" he was able to conceal his substance. And so on.

It is probably inevitable that major developments in history should be described not only by a few major writers but also by many minor ones. Scholars, apprentices, and charlatans all compete for space, and pathbreaking discoveries and conceptualizations are often drowned out by discredited theories repeated over and over. It is therefore with some trepidation that one picks up yet another volume on the origins of Zionism. After all, the large summations by Laqueur and Halpern, both in English, are less than ten years old. What else is there to be said so soon after the appearance of these works?

David Vital concentrates on the nineteenth century. He stops in 1897 where most other investigators barely begin. In his four-hundred pages, he makes more use of Hebrew sources than will probably be found in any other English-language volume. He sheds more light on the cultural background and personality structures of the Zionist founders than any of the other researchers who have surveyed this particular scene. But these are marginal considerations at best. Vital's work is important because it has a special quality. Reading *The Origins of Zionism*, we can actually see the Jewish situation in the emancipatory era; we can grasp what the founding Zionists were observing, thinking, feeling. Vital is convincing, his account rings true. The chapters on the improbable Dr. Herzl are an unexcelled presentation of the psyche and manner of the man who galvanized the Zionist movement into action. Of course, David Vital has a comprehensive knowledge and deep understanding of Jews, both East and West—and that is rare by itself. He has a subtle and supple mind, and he can bid his words to do his will. In short, Vital is a master. At last a major writer has done justice to a major topic, and the work shines like a diamond in a heap of stones.

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KLAUS MISGELD. *Die "Internationale Gruppe demokratischer Sozialisten" in Stockholm, 1942–1945: Zur sozialistischen Friedensdiskussion während des Zweiten Weltkrieges.* (Acta Universitatis Upsaliensis, Studia

Historica Upsaliensia, number 79.) Uppsala: Historiska Institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell International, Stockholm. 1976. Pp. 216.

The discussion circle known as the international Group of Democratic Socialists coalesced in Stockholm during mid-1942. Socialist exiles from Germany, German-occupied countries, and Allied nations met with Swedish party and union leaders. The participants, including Myrdal, Sterner, Tarrow, Brandt, Tranmael, Szende, Kreisky, Böhm, Karniol, Guesde, and Paul, attempted to define a new European order for the postwar world. Three themes predominated: the place of Germany in an integrated Europe, the role of socialist-worker internationalism, and the nature of the peace. In response to Allied declarations and practices, especially the Atlantic Charter, recognizing Darlan and favoring "Vansittartism," this group attempted to place democratic socialism and labor internationalism on the agenda of postwar reconstruction. By March 1943 the group had defined its principles and on May Day 1943, in conjunction with Swedish socialists and unionists, publicly presented "The Peace Aims of the Democratic Socialists." The lack of response to its most significant proclamation illustrated the group's isolation. The long-range significance of this "little Internationale" lies in having provided alternatives to the Allies' as well as the other exiles' policies, and in having provided models for individuals such as Myrdal, Brandt, or Kreisky during their later political careers.

The first study devoted to the International Group defines the influence of Norwegian exiles in the group's emergence, but the main concern is a "content analysis" of the "Peace Aims" (pp. 62-95). The author traces the Swedish influence and the sources of the declaration's compromises, and illustrates how participants later excerpted from the declaration, in particular Brandt and Paul during the Cold War. While Klaus Misgeld does not print a copy of the nine-page declaration, available only in Swedish, he thoroughly documents the group's attempt to set a new labor international within a collective security system. The group sought alternatives to the Western Allies' "revengeful" approach, to the Soviet and Communist exiles' refusal to make specific postwar commitments, and to the "official" exile groups' fatalistic acceptance of Allied policies (especially the London executive of the German Social Democrats). Misgeld shows that by late 1944 optimism disappeared as Allied activities excluded moderate socialist proposals, even when they derived more from liberal than Marxist principles.

The valuable research on exile politics lies buried in a dull and unimaginative book. The author

presents the history of political ideas in a cramped style and format which prevents anyone from guessing that individuals caught in the cross-fire of fascism were seeking desperately through these exile organizations to create a securer international and social environment.

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DONALD L. M. BLACKMER and SIDNEY TARROW, editors. *Communism in Italy and France*. Princeton: Princeton University Press. 1976. Pp. xii, 651. \$25.00.

The nature and future of Eurocommunism is a subject of chronic speculation. This work, *Communism in Italy and France*, edited by Donald L. M. Blackmer and Sidney Tarrow, confronts the complexity of two major nonruling Communist parties and dispels many myths about supposed prototypes for such parties. The book results from the collaboration of many scholars in the field of nonruling Communist parties to structure a framework that would allow for comparison between the French and Italian parties as political parties. The approach is that of political science/sociology set in the context of the historical and contemporary development of both countries. The work achieves the goal expressed by Blackmer: "We hope that in this volume we have conveyed a richer sense of the dynamic internal reality of each of these parties and of their interreaction with the wider environment around them."

The idea originated with the editors ten years ago and evolved with the aid of the Planning Group on Comparative Communist Studies. The book is a compilation of edited or revised papers originally presented at a Planning Group conference on the French and Italian Communist Parties held at M.I.T. in October 1972. The papers have been melded into a cohesive whole in an attempt to achieve as thorough a comparative analysis as possible.

There is obvious merit in this concerted approach to the many aspects of how these Communist parties integrate into their native political systems. The data itself is exhaustive and the conclusions are challenging and convincing. Although no master plan for the analysis of every nonruling Communist party is suggested, the method and prospectus should have a degree of transferability to students of other Communist parties.

The reading is thought-provoking. In his conclusion Tarrow declares, "What is the fate of the revolutionary party in a non-revolutionary situation? This is the underlying question raised by the contributors to this volume." The conclusion is

that experts cannot forecast. One must balance the realism of this judgment with that expressed by Blackmer: "Their activism, their official optimism, and their self-conscious attention to matters of strategy and organization create a sense that the French and Italian Communist parties are more nearly masters of their fate than other parties." Ironically, one might muse, the social scientist pursues the dialectician.

The book substantiates the theory that, despite the Moscow connection, each party has a strong national identity. Scholars have been slow to explore in biography the unique personalities of individual Communist leaders, and indeed in this case the unique national identity of each nonruling Communist party. This book is a step in demystification that is overdue.

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W. B. STEPHENS. *Sources for English Local History*. Totowa, N. J.: Rowman and Littlefield. 1973. Pp. x, 260. \$6.50.

JOHN RICHARDSON. *The Local Historian's Encyclopedia*. Reprint. New Barnet, Herts.: Historical Publications. 1975. Pp. iii, 312. £2.50.

The professional development of English local archives in the last twenty-five years is a striking achievement which has made possible enormous strides in English local history. For all too long local history was a blend of the curious and the quaint, the preserve of the Victorian vicar and the leisured antiquarian and only rarely of the scholar of vision who recognized that national history was complete only in the context of and as a complement to the development of many local communities. Before 1950 only one county had a modern printed guide to its records, the pioneering work by F. G. Emmison for Essex (1947-48), so that aside from the monumentally conceived (but as yet incomplete) *Victoria History of the Counties of England* (the VCH) and a number of constitutional studies of national scope with local examples, the development of local history as a subdiscipline lagged behind advances in other areas. Now most record offices have printed guides. The training of archivists has taken a quantum leap, and most county archivists have training in history (some with doctorates). Special university programs are also growing—among which Leicester has pride of place.

American scholars were among the first to publish significant modern monographs in English local history. At Harvard W. K. Jordan's work not only led to his own publications on philanthropy, charities, and local government, but also helped inspire Wallace MacCaffrey's *Exeter, 1540-1640*

(1958) and Thomas G. Barnes' *Somerset, 1625-1640* (1961). In England W. G. Hoskins had begun work on Devonshire which would launch him on his way to an outstanding career in the field and to inclusion in an Honours List in this decade "for services to local history." The growth of publication in English local history is reflected in the issues for 1976 of this journal, in which monographs on six different counties, three boroughs, and one village community were reviewed. Yet few American scholars work actively in the field in comparison to the growth of interest in local history in England. Of course, the incentive to investigate one's own surroundings is lacking, and access to records may be difficult, but perhaps the relative lack of interest is also in part a reflection of the rather rigid chronological definition of American academic posts in English history. Regardless of the reasons, the appearance of these two useful books in local history should help spur a greater awareness of the dimension which local sources can add.

W. B. Stephens' *Sources for English Local History* is a remarkable achievement. His introductory chapter is a tour de force, covering the necessary general sources: the printed original records both national and local, the VCH with its mine of information and inherent limitations, the wealth of the *Parliamentary Papers*, the broad and often frustratingly summary surveys of the Historical Manuscripts Commission, and the central clearing-house for information on local records, the National Register of Archives. Stephens keeps his text uncluttered and relegates full citations to the footnotes which thus at times run to over half the page. Because many of the sources present problems—the over 8,000 volumes of *Parliamentary Papers*, for example, have multiple methods of citation and pagination, and many statutes of local import are not collected—Stephens also makes reference to articles and books which prepare a scholar to use the sources fruitfully.

The remainder of the book is divided into seven chapters: population and social structure, local government and politics, poor relief (including charities, wages, and prices), industry and trade, agriculture, education, and religion. The section on agriculture is divided chronologically at 1750, the earlier part indicating sources for *Domesday Book*, subsidies, extents, deeds, and the like, the latter introducing land tax records, tithe maps, the county by county surveys produced by the Board of Agriculture, and crop returns. The other six chapters are topical in nature, so that under local government there are subsections on the manor, parish, town, and county, while industry is divided into domestic and international sectors as well as communications, where sources for canals, turnpikes, and railroads are discussed.

Many of the areas which Stephens surveys require the historian to have special technical skills. Because there are only incomplete sources for estimating population before the nineteenth-century censuses, one has to know the manner in which records such as subsidies, parish registers, muster rolls, hearth taxes, and poll books were prepared and then how to apply "multipliers" to arrive at a total population. Stephens sketches briefly the suggestions of leading centers such as the Cambridge Group for the History of Population and Social Structure, and guides one to articles in which the adequacy of these demographic devices is fiercely debated. There are other useful discourses of this type, for example on prices and wages.

Sources for English Local History is valuable not only for its breadth but also because Stephens presumes neither too much of someone just entering an unfamiliar field of historical investigation, nor too little of the scholar who is assumed to have an adequate knowledge of chronology and institutions. The topical arrangement allows him to cite in one place a diverse number of sources so that classes of documents in the PRO are juxtaposed with citations from the *Parliamentary Papers* and secondary writings as well. Stephens' approach thus differs from G. R. Elton's *England 1200-1640*, where all the sources produced by a given institution are grouped together. Yet both approaches succeed, in part because local history spans a wider number of themes, in part because record-keeping in non-governmental circles became more important after the Civil War as the dominant influence of the crown and Church lessened.

John Richardson's *Encyclopedia of Local History* is a curious blend, at most times a treasure-house of information not readily accessible, at times a source of disappointment. As a sourcebook of facts of great use to the local historian it is impressive. The areas included by Stephens are of course covered and a number of others added: archeological terms, public utilities and services, the local militia, architectural terms, place names, coins and tokens, and heraldry. Each section is broken into a number of parts, so that "Land and Agriculture" has sub-sections on land measurement, field names and geographical features, open-field farming and enclosures, tenants and tenures, the transfer and inheritance of land, agricultural history, farm houses and buildings, and miscellaneous. Within each there is an alphabetical organization of terms and definitions, often preceded by a general introduction.

Richardson's work thus is a valuable *vade mecum* which will spare the researcher into local records the frustration of having to suspend temporarily a search to chase down a query over terms, a question on a detail of landholding or feudal dues, a

description of an element of church fabric or ecclesiastical nomenclature, or an explanation of some process in a complicated legal maneuver. He has also supplemented the essentially alphabetical approach with two other useful features, a number of chronological sections which trace a line of development (agricultural inventions, a series of statutes on some topic, grants of markets and fairs in the different counties, the new features incorporated into successive censuses) and lists of organizational importance (the founding dates of important schools, prisons, hospitals, and utility companies). The section on railroads is valuable, for example, not only for the date of a particular company's creation but because the railroad into which it was merged is also given.

The disturbing thing about the *Encyclopedia* is the deceptively thin elaboration in many cases. There are innumerable complex topics in local history, but Richardson's succinct definitions for "parish" or "borough" or for many ecclesiastical terms mask the more involved development. Had references been appended in place, indicating that a fuller elaboration was necessary, the problem would have been lessened. Although in some cases these notes are present, most of the important sources are relegated to a bibliography at the end of the work.

These two books differ in approach and in scope, but together they are valuable tools to help the historian expand his or her professional horizons. A better awareness of the riches of local history can add an important dimension in historical writing.

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NICHOLAS P. CANNY. *The Elizabethan Conquest of Ireland: A Pattern Established, 1565-76*. New York: Barnes and Noble. 1976. Pp. xi, 205. \$18.50.

In a brisk monograph, full of ideas, Nicholas Canny takes a critical look at eleven years, 1565-76, in Irish history. He sees the program-making of Sir Henry Sidney (though he was not continuously lord deputy of Ireland) as the moving influence on Elizabethan policy in Ireland during these years. Basically Sidney believed that Elizabeth should take effective responsibility for the whole island. This concept was the "Pattern Established" which involved England in the long conquest which ended only in 1603. Sidney's other key plank was partial recolonization. Insurrections in Ulster and Munster put Gaelic and Old English land potentially at his disposal. Their lords could be removed, but that removal left behind a potential labor force which he considered might be usefully employed to operate English-run estates.

The perceptive chapter on Gaelic society with

which the book opens indicates some of the problems which were involved in turning such a traditionally organized society—a somewhat archaic one in its Western European setting—to serve English objectives. The prestige of birth, rather than possessions, helped to render Irish society peculiarly cohesive. If one part—the apparent head—was lopped off then it did not follow that what remained was helpless and amenable to orders; if nothing else it was too mobile to be controlled. Removal of the upper orders and the military hierarchy did not mean that a servile, hardworking laboring class would accept new masters and would be available to new English landholders to exploit efficiently.

Against this background Canny's examination of successive colonizing plans and attempts of the years from 1568 onward is precise and revealing. A great outpouring of plans and action produced little or nothing in the end; that they took place was, however, to prove significant. Sir Thomas Smith sent no less than three expeditions comprising 550 men (compare the scale of the Virginia enterprises of 1606–08) to occupy the Ards (part of modern County Down), but somehow they never built the urban nucleus Elizabetha (their Jamestown) and so far failed to establish a subservient native labor force that some of their workers turned on Sir Thomas' illegitimate son and killed him. Sir Thomas was original in his use of a printed broadsheet asking for subscriptions, a map showing the location of the lands, and a dialogue-style promotion pamphlet (in all those anticipating the Virginia Company).

Sir Thomas was a portent but a failure. The Earl of Essex who followed him in Antrim, from 1573 to 1576, had a more specific mission, to ward off the danger of Scots infiltration. He was given government money to pay his settlers in their role of soldiers, but rather than settling down they became merely a military retinue for Essex, who took some of the duties of an Ulster president, without a presidency organization, until his death by poison in 1576. By that time it was evident to Sidney that colonizing plans in Ireland should be put in cold storage where they remained for nine years. Canny recounts all this and indicates that the intermission in Irish colonizing plans enabled Sir Humphrey Gilbert and Sir Walter Raleigh to develop projects on very similar lines but with the objective changed to North America.

Canny sees these years—and this is perhaps the controversial aspect of his monograph—as ones in which English attitudes toward the Gaelic-attached Irish hardened. They were, after the experiments of the Sidney period, seen as inferior barbarians, good for labor but not as acceptable co-subjects of the Queen. He sees these pre-

suppositions dominating later Elizabethan attitudes to the Irish, and he sees them carried over into the Englishman's attitude toward the Amerindian he encountered in the second stage of his colonizing peregrinations. The Indian, who was also the predestined laborer from a barbarian society, proved resistant to pressures to make him plow and sow, and so his status was reduced further to that of human game, fit only to be picked off by gun or sword from the lands which he uselessly occupied or failed to work on for the support of others. Canny's view is very much in line with that in Francis Jennings' recent book, *The Invasion of America* (1975), and may set off similar discussion in regard to English attitudes to and actions toward the Irish.

The book makes a valuable contribution to the understanding of at least one subsection of the Elizabethan period in Ireland, but it throws out hints of much wider relevance for North America no less than Ireland. We can look forward to further contributions from Canny which may in the end add up to a full analysis of the much larger bundle of attitudes, objectives, and techniques which aligned the Ireland of 1576 to 1620 beside North America in the same period.

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T. S. WILLAN. *The Inland Trade: Studies in English Internal Trade in the Sixteenth and Seventeenth Centuries*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 154. \$15.00.

T. S. Willan has published here several essays on the history of transport and merchandizing from, roughly, the accession of Elizabeth I to the death of Charles II. Unlike foreign commerce, which is richly documented in the port books, domestic trade in the early modern period has left historians with few records. The unifying element of Willan's study is the important and hitherto neglected theme which he has explored: whether traditional domestic marketing patterns changed as a result of the Renaissance inflation.

During Shakespeare's time improvements in all three modes of transport—overland carriage, river traffic, and the coasting trade—encouraged the wholesale traffic of wool, grain, and coal along arteries to the capital, and exported goods were returned along the same routes. Beyond this stage less is known about the retail of goods outside towns. Local markets sold the natural products of their neighborhood. Fairs were periodic, tinkers and pedlars peripatetic, irregular entrepreneurs of distribution. During the seventeenth century provincial craftsmen began to absorb imported goods such as groceries and tobacco into their stocks.

Throughout the seventeenth century the number of these shops was increasing. Willan's analysis of tradesmen's tokens issued and distributed between 1649 and 1672 indicates shops in at least 822 places, many without markets. Shopkeeping had increased because of the decline of other trades, especially the cloth trade. "Husbandmen, labourers, and artificers . . . have left off their working trades, and turned shopkeepers," it was reported in 1681. These new tradesmen could no longer afford to regard their retailing as pin-money. Their inventories became more extensive, reflecting the rising imports of luxury goods after the Restoration. As prices fell because the population remained stable, new consumer habits developed among the middling sort and rustics. Country people who had formerly exchanged their goods in market towns and bought their needs in urban shops now found their wants supplied at home. If England were not yet a nation of shopkeepers, these patterns of retailing had evolved before the commercial expansion of the eighteenth century and the Industrial Revolution.

Willan's book is necessarily uneven in its range and tentative in its conclusions. Only such an intensive analysis of the sources could have identified the difference between shops and shopping, much less coordinated the economic network of the transition of craftsmen to tradesmen.

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LAWRENCE KAPLAN, *Politics and Religion during the English Revolution: The Scots and the Long Parliament, 1643-1645*. New York: New York University Press, 1976. Pp. xxvii, 176. \$15.00.

Recently a number of historians have presented reinterpretations of the politics and management of the Long Parliament. Those concentrating on the period after Pride's Purge have reached a state of amicable basic agreement, but their colleagues concerned with the earlier stages continue to disagree with each other, sometimes over the very categories of analysis. While most have replaced S. R. Gardiner's religious "parties" with some variation on J. H. Hexter's secular "groups," each historian has presented his or her own scheme for the period following the death of John Pym. This part of the "English Revolution," at least, remains a battleground.

Lawrence Kaplan's spare, extended essay makes a significant contribution toward the correction and replacement of Gardiner's account. Based upon a wide range of manuscript and printed sources, including extensive use of the parliamentary diary of Sir Simonds D'Ewes and of the

Thomason Tracts, the book centers firmly on Westminster and focuses on members of the Commons. It displays a number of virtues not shared by all of the competitors. The author is both properly skeptical of unsupported comments recorded by memoir writers and by foreign diplomats and unwilling to stretch every innovation into a "radical" or "revolutionary" act, every criticism of a Lord into an attack upon the upper house. Such commendable discretion makes for an account in which events appear to move at their own pace.

Extending Hexter's categories, Kaplan argues that the "middle group" disintegrated after Pym's death and was succeeded by a "war party" led by Sir Henry Vane the younger, Oliver St. John, and Sir Arthur Haselrig and by a "peace party" headed by Denzil Holles, Sir Philip Stapleton, and Sir William Lewis. He traces the alliance of the Scots first with the "war party"—from 1643 to September 1644—and then, after six months in limbo, with the "peace party"—from March 1645 onward. In addition, he ties down the origin of the political use of the terms "Presbyterian" and "Independent" to the Scots and disposes of Vane's supposed plot to depose Charles I in 1644. The categories employed, however, will not satisfy all historians. The switch from "group" to "party" takes place with neither definition nor explanation. It seems more than ironic to find such parliamentary commanders as the Earls of Essex, Warwick, and Manchester in the "peace party" and such stay-at-Westminster peers as Viscount Say and Sele and Lord Wharton in the "war party." While Kaplan's book does not end the debate, then, it does present a careful, plausible account of the political maneuverings of the parliamentarians and their Scottish allies during the period 1643-45.

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ALAN MACFARLANE, editor. *The Diary of Ralph Josselin, 1616-1683*. (Records of Social and Economic History. New Series, number 3.) London: Oxford University Press, for British Academy, 1976. Pp. xxvi, 727. \$44.00.

Ralph Josselin was a country clergyman, a Cambridge graduate of yeoman stock who became vicar of Earl's Colne in Essex in 1641 when he was twenty-four and remained there until his death in 1683. In August 1644 he began to keep a detailed diary; for the next twenty years he normally made several entries a week. In the mid-1660s the entries became briefer and less interesting, but he continued to the end, carefully noting the names of the ministers who preached for him during his last

illness. The diary is extraordinarily full—it runs to some 290,000 words—and the only previous edition, that of the Camden Society in 1908, contains only about one quarter of the text. So this complete version is very welcome, especially to those interested in the social, economic, and religious history of mid-seventeenth-century England.

Josselin had all kinds of interests. A keen observer of the political kaleidoscope, he was a good Parliamentarian in the Civil War, though we was dubious about the execution of the king. Besides being a clergyman he was also a schoolmaster, a book collector, and a fairly successful farmer. Like all farmers he was much concerned with the weather and the price of seed; both are carefully and frequently recorded, as are the state of the harvest, his debts and his loans, and his gradual accumulation of property. Josselin was a devoted family man. His wife, who outlived him by ten years, presented him with ten children, seven of whom survived to maturity. His health, and that of his family, are constant themes of the diary, and serve as a forceful reminder that our ancestors suffered most of the time from a collection of physical ailments from which we are mercifully free. His dreams fascinated Josselin, who describes many of them in almost incredible detail. He was an orthodox Calvinist with Congregationalist leanings, in the 1650s he became very much taken with millenarian ideas. The Restoration made him very uneasy. He did not subscribe to the Act of Uniformity; but, to his own considerable surprise, he somehow managed to avoid being deprived of his living.

Alan MacFarlane, the editor, suggests at the end of his introduction that Josselin may well rank as one of the greatest English diarists of all time. He is not that, if only because his mind is not as interesting as, say, Pepys'; he is not a distinguished stylist, and after a time one begins to weary of his constant benediction of the Lord. But there is a mine of information here on far more matters—economic, political, religious, and personal—than is customary in seventeenth-century diaries. MacFarlane himself has already put it to excellent use in his monograph, *The Family Life of Ralph Josselin*. The editing is very fine, although the notes are occasionally briefer than one would wish and the lack of a subject index is most regrettable.

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These are the first two of eight volumes of correspondence to be published by the Clarendon Press as part of its fine, critical edition of Locke's works. They cover the period from his matriculation at the age of twenty in 1652 to 1686, when Locke was an exile in the Netherlands. Many of the letters are expressions of courtesy, acts of duty, and notes of apology. Individually, they often lack interest and are sometimes tiresome. Taken together they form the principal source for Locke's biography. They are a guide to his own education, to many of his friendships and an abortive love affair, and most particularly to the social and intellectual circumstances of his widening circle and of his class.

These volumes contain little that bears upon seventeenth-century English politics—almost nothing on the crises of the early 1680s, a few references to Stillingfleet, and no discussion of the composition of the *Two Treatises*. Shaftesbury appears, but not as the leader of a political inner ring. The most interesting letters, though previously printed for the most part, reveal Locke's early views on the law of nature and the civil magistrate (nos. 75 and 81) and his appreciation of toleration in Cleves (no. 175); they also include a reflection on recreation and labor (no. 328) and essays on things indifferent to God (nos. 374 and 426). The series of letters to Edward and Mary Clarke form a first draft of *Some Thoughts concerning Education*. Locke is an excellent observer of medical diagnosis and practice. His letters on scientific measurement and observation throw light on contemporary natural philosophy, though there is little on the *Essay*. Equally important, the correspondence with Justel, van Limborch, and especially Toinard vividly illustrates Locke's participation in what he called the "Commonwealth of Learning" of the early Enlightenment.

E. S. de Beer's edition is a model of erudition. It offers useful finding lists, translations, and learned notes, along with an elegant critical apparatus which aids the reader and never interferes with reading. One could hardly ask for a better scholarly edition. At the same time, it is so expensive as to be beyond the means of most individuals who will use the letters and of many libraries as well. The price may remind both buyers and readers of Locke's quip to Boyle, that he was "now upon a new sort of chemistry, i.e. extracting money out of the scholars pockets" (no. 223).

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JOHN LOCKE. *The Correspondence of John Locke*. Volumes 1 and 2. Edited by E. S. DE BEER. (The Clarendon Edition of the Works of John Locke.) Oxford: Oxford University Press. 1976. Pp. xcvi, 707; vii, 805. \$55.00 each.

DAVID H. HOSFORD. *Nottingham, Nobles, and the North: Aspects of the Revolution of 1688*. (Studies in British History and Culture, volume 4.) Hamden, Conn.: Archon Books, for the Conference on British Stud-

ies and Wittenberg University. 1976. Pp. xvi, 182. \$12.50.

"The lack of attention the north has received," says David Hosford in introducing his excellent study of the Glorious Revolution, "is not surprising, for in many ways it represents something of an historical 'if.'" If William had landed on the Yorkshire coast instead of at Torbay, he might have acquired a considerable, if not substantial military following. His march on London also might have looked less like a progress, more like a campaign. Could the Revolution of 1688 then have been called "the bloodless"? Surely the Duke of Newcastle's men would have caused at least an incident. Other "ifs" raised by Hosford's superb reconstruction might include James's lost chances for retaining the crown. Had he strengthened and exploited his pockets of support, wooed the wavering (which group included many Whigs), or given the impression of extending a true toleration instead of alienating his Tory support, James II at least might have embarrassed his son-in-law by holding a portion of the North against him. The early *quo warranto* proceedings against the city of Nottingham, Hosford discovers, were not incompatible with the city's desires. Had they been handled with greater diplomacy, could the Nottingham rising have been averted?

In any case, Hosford sees William and Mary's incumbency as inevitable, though he indicates impressive unexploited potentials for both Williamites and Jacobites in the North. That the Williamite northern nobles, with the last-minute assistance, surprisingly, of the Princess Anne, were able to hand over their shires to the new regime certainly made William's a national victory. Yet the character of Hosford's data, in many aspects amounting to little more than assemblages of names, has led him to construct a Namierite revolution. He insists that 1688 was not a national, but a nobleman's triumph. This view has placed him in league with J. R. Jones and John Kenyon in insisting on the conservative objectives of the Revolution. Furthermore, Hosford strongly implies that 1688 was particularly British—perhaps even merely English—rather than European in inception and scope. Nonetheless, within his own superb range of manuscript material, Hosford has given us a wealth of local detail far greater than has appeared in any previous study.

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G. V. BENNETT. *The Tory Crisis in Church and State, 1688-1730: The Career of Francis Atterbury, Bishop of Rochester*. Oxford: Clarendon Press. 1975. Pp. ix, 335. \$26.00.

There have been various attempts to portray the career of Francis Atterbury, Bishop of Rochester, beginning during his lifetime. But even the most competent of these was little more than a journalistic sketch. The difficulty, of course, has been a total lack of basic evidence. The British Government, the Jacobites, and the families of those surrounding Atterbury all had good reason for refusing to disclose information. Working chiefly in the Stuart and Harley papers as well as the archive of Robert Walpole in Cambridge University Library, Garry V. Bennett has produced a truly significant book, which is more than a biography. The author has brilliantly penetrated beyond the slogans of Tory and Whig, High and Low Church, and has discovered the real nature of the Anglican crisis after 1688. He asserts that it was necessary that this study take the form of a biography, "for at every step the story of Anglican Toryism marches parallel with the career of this most remarkable churchman." There are several logical reasons for the author to approach the story in this way. First, Atterbury's career is of a political importance equaled by that of no other Anglican divine, with the possible exception of Jonathan Swift. For thirty years he was the prime mover and champion of the High Church cause and was closely involved in many of the political events of the era. Secondly, the colorfulness of his personality, with its many flaws as well as gifts, imparts to the story not only pace and excitement but also humor and something of the bizarre.

Francis Atterbury was educated at Westminster School, Christ Church, and while at Oxford attracted a great deal of attention by editing and translating several literary works. But he was soon engaged in the more serious work of defending the Church of England against attacks by James II. After the Revolution of 1688, though trained in the doctrine of non-resistance and passive obedience, Atterbury swore fealty to William and Mary, won a reputation as an excellent preacher, and soon gained an appointment as one of the royal chaplains. After the accession of Queen Anne he was appointed Dean of Carlisle.

Atterbury's fortunes naturally declines after the Whig party came to power, but he did take the lead in creating a High Church Party which deliberately thrust religion into the political arena. Atterbury refused to accept the fact that Dissent had become a permanent feature of English life. When, in 1710, the prosecution of Dr. Sacheverell produced an explosion, he was among the most active of those pamphleteers who inflamed the nation against the Whig ministry and Whig Parliament. When the ministry fell, rewards naturally followed. Queen Anne appointed him Dean of Christ Church, Bishop of Rochester, and Dean of West-

minster. Had his party remained in power, there is little doubt that he would have been named Archbishop of Canterbury. During these years Atterbury struggled to establish High Church hegemony within Convocation and strove unsuccessfully to commit such leading Tory politicians as Harley to a reform of the Church.

Although disappointed in the accession of the House of Hanover, he took the oaths to that House and helped officiate at the Coronation. He soon became an enemy of the government, however, and, when the Rebellion of 1715 broke out, refused to sign the paper attaching himself to the Protestant succession. In the summer of 1716 with the Jacobite cause at its lowest ebb, the Jacobites selected the Bishop of Rochester as the man to organize their work in England. For the insurrection of 1720, the streets were to be barricaded; the Tower and the Bank were to be taken over; King George and his family were to be arrested, and King James was to be proclaimed. The design became known to the Duke of Orleans, regent of France, who was on friendly terms with the House of Hanover. He put the English government on its guard. Some of the leading conspirators were committed to prison; and among them was Atterbury. No bishop of the Church of England had been taken into custody since 1688.

Bennett claims that despite all the accusations levelled at him, Atterbury had not been a Jacobite before 1716. He certainly had no romantic or theoretical attachment to the Stuart cause. His loyalty was to the Church of England and to a vision of its place in English life and society. Jacobitism to Atterbury was a means not an end in itself. If the Tories had managed to survive after 1714 as a powerful Church party he would certainly never have embraced the Jacobite cause at all. The Hanoverians had become linked in his mind with the destruction of the Tory party and the decline of the Church of England. Atterbury's tragedy was that in spite of his great religious and pastoral gifts, he was convinced that the well-being of the Church of England was still primarily bound up with the political regime and that its cause was best furthered by political agitation. It was a desperate expedient.

Atterbury could only be reached by a "bill of pains and penalties." Such a bill the Whig party, then in the majority, was quite prepared to support. Many Tories began to relish the prospect of another Sacheverell trial, and the Government was clearly worried. When Parliament met, the evidence against the bishop was laid before committees of both Houses. In the Commons a resolution pronouncing him a traitor was carried by nearly two to one. Atterbury, though invited to defend himself, chose to reserve his defence before

the House of Lords. There the contest was much closer but the Lords passed the bill by 83 votes to 43. He was to be deprived of his spiritual dignities and banished for life. No British subject could hold any intercourse with him except by royal permission.

Atterbury left England in 1723, and, after a short stay at Brussels, took up residence at Paris. He became the leader among the Jacobite refugees who were assembled there. He was later invited to Rome by the Pretender, but declined. He soon left Paris, gave up politics, and until his death largely devoted himself to his studies.

Any final judgment on Atterbury is difficult. As Bennett points out, Atterbury's personality, restless intelligence, and subtle ingenuity were above question. He was equipped for the public persuasion and private maneuvers involved in the business of politics, yet his character was flawed. His political judgments were in the long term faulty, and his influence was one which the Church of England learned bitterly to regret. As the author states: "By defect of vision and a flawed personality he hastened the events he most feared."

The excellent bibliography is up-to-date and complete and should prove a help to the beginner as well as to the specialist. The fact remains that history cannot help being concerned with people and that people as individuals are interesting, to writers as well as readers. Thomas Atterbury, Bishop of Rochester, is certainly a case in point. It is only to be hoped that the prohibitive price of this volume will not deter prospective purchasers.

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B. W. HILL. *The Growth of Parliamentary Parties, 1689-1742*. Hamden, Conn.: Archon Books. 1976. Pp. 265. \$15.00.

Brian Hill, the author of a perceptive synthetic essay "Executive Monarchy and the Challenge of Parties, 1689-1832" (*The Historical Journal*, 1970), now offers us a detailed account of the development of parliamentary political parties in the reigns of William and Anne and of their gradual decline under the early Hanoverians. His is chiefly a narrative approach, and in this way the book fills a gap between a mass of monographic material and a paucity of good texts even though the author has eschewed any extensive consideration of electoral politics.

Nonetheless, two caveats must be entered. The first concerns Hill's assessment of the importance of party differences in the age of Walpole. In his earlier essay he suggested that "although the survival of the Whig-Tory division after 1714 must be

taken into account, the dominant theme until about 1770 was that of court and country" (*The Historical Journal*, 1970, p. 387). But in this work he seeks to overturn this interpretation and concludes that "in 1742 the Tory party not only was still in existence and in good heart . . . but also remained an important and effective part of the political scene" (p. 278). This may be a needed corrective to those scholars who predate the demise of the Tory party as a significant political force at the national level, but specialists in early Hanoverian politics may well feel that the truth lies somewhere between Hill's earlier and current judgments.

The second caveat has to do with the accuracy in detail of Hill's narrative. While I do not wish to take issue with the broad outline of the politics of William III's reign that he presents, I am a little uneasy about the number of factual errors and misstatements that have crept into the chapters covering the years I know best. The following is a list of corrections only of demonstrably mistaken assertions: the Earl of Nottingham had held office before 1689 (p. 36); Lowther and Goodricke did not become court managers until 1690 (p. 40); Sir John Guise was not a privy councillor in 1689, and various Whigs did oppose the attack against Halifax in August 1689 (p. 41); Sir Stephen Fox was not Carmarthen's lieutenant and Carmarthen was not "undisputed" as leading minister in the spring of 1690 (p. 48); Paul Foley, not Charles Montagu, proposed the million scheme of 1692-93, and the scheme failed to be fully subscribed (p. 58); Sir Edward Seymour had opposed the court over excises in 1693-94 (p. 60); Sir Edward Harley had not been out of Parliament for six years by 1695 (p. 65); Thomas Pelham was not a member of the Treasury Board in 1696 (p. 70); the 1700 Irish resumption bill did not exclude customs officials from the Commons (p. 82); Sir Charles Hedges was not a nominee of Nottingham in 1700 (p. 83).

With these qualifications, Hill's book can be recommended to teachers and students of later seventeenth- and early eighteenth-century English politics. Now, all the publisher must do is to make the work accessible to them in paperback.

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G. E. MINGAY. *The Gentry: The Rise and Fall of a Ruling Class*. (Themes in British Social History.) New York: Longman. 1976. Pp. xii, 216. \$15.00.

An aged English country gentleman thought the most dangerous thing about Hitler was that he did not understand about "Houses." From feudal times until well past the Industrial Revolution, Britain was ruled from Houses, and by far the largest proportion of those establishments belonged to the gentry. Few are better qualified than

G. E. Mingay to review and condense modern research on this topic. His book is an introductory overview rather than a new foray into the primary sources of estate management or social control. Mingay's chronological range is broad, and the account is interspersed with discussions of sport, law, politics, and architecture. He is more comfortable dealing with the eighteenth and nineteenth centuries but makes a worthy attempt to cover earlier periods, including the perilous reefs of the "rising and declining gentry" controversy.

Mingay emphasizes "the essential stability and conservatism of landed society" (p. 57), and at the same time repeatedly alludes to the gentry's "willingness to seize the economic opportunities of each age as it advanced" (p. 10). He sees the secret of its remarkable ability to survive and thrive in its practice of natural selection through upward and downward social mobility. He rightly points to the importance of the tenant-landlord relationship, based on a real sense of trust and mutual economic advantage, which helped to preserve the social and political order.

The tone of this book is traditional and conservative. For example, Mingay sees the Civil War as a political and religious struggle and de-emphasizes its economic and sociological antecedents. He praises the role of the gentry as local governors who provided unpaid, occasionally selfish, but generally humane administration and justice. He believes that they served as useful buffers between the peerage and the common people.

Mingay's definition of the gentry as a class remains vague and unsatisfactory, as perhaps it must be. However, obscure lines of division between the upper gentry and the poorer peers on the one hand and the lower gentry and the substantial yeomen on the other can vitiate much of what is said about the group as a whole. Moreover, parliamentary historians would disagree with the author's contention that the peerage and gentry "generally thought and spoke in almost identical terms" (p. 188). Indeed, his political generalizations are sometimes superficial and misleading. Some will find his benign view of enclosure too sympathetic. Nevertheless, one cannot dispute Mingay's judgment, especially after examining the continental experience, that England has "not been unfortunate in her ruling class" (p. 134). This book provides an excellent introduction to the study of part of that class.

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PAUL LANGFORD. *The Eighteenth Century, 1688-1815*. (Modern British Foreign Policy.) New York: St. Martin's Press. 1976. Pp. vii, 264. \$15.95.

Searching for policy in eighteenth-century English politics can be a creative art, as Paul Langford's knowledgeable narrative on British foreign policy indicates. His purpose is to consider the relatively neglected subject of foreign policy in light of the wealth of recent literature in his specialty, English domestic political history. Inevitably he finds more politics than policy but struggles bravely, and often suggestively, in search of the latter.

Perhaps a story that ends with Britain at the Congress of Vienna must be a success story, but how much of Britain's power in 1815 was based upon diplomatic victory during the preceding hundred and thirty years? More explicit discussion of Britain's relative economic, fiscal, and military strength would be needed before a verdict on the success of its foreign policy could be reached. Did Britain's foreign policy makers achieve influence commensurate with the nation's usable strength?

Langford's theme, that British policy-makers did not overvalue Europe or undervalue empire, seems fair only if clear preoccupations of both kinds can be said to neutralize each other. Britain went to Europe for monarchs, then blasted them for being European. Bute's peace, which here receives the same rehabilitation that is evident in Ronald Hyam's recent essay, may well have secured enough European tranquility to leave England out of Europe and unable to find a worthy ally against France for a generation. Britain's commitment to Europe could be more, or less, than Langford's "limited liability."

Perfidiousness is rejected as a theme, perhaps a little too defensively. With two notable exceptions, Britain made war and peace when she wished to. As her foreign policy seldom ensnared her in continental commitments that were not currently useful, she seldom made war alone and she often made peace alone. That, too, was diplomatic success.

Seeing European diplomacy in the context of British politics may be too customary a perspective for English history students, and too obtuse an angle for many others, but this essay does more than lead the neophyte across the Channel. It would be unfortunate if the apparent absence of copyediting, and the poor maps and index, masked the merits of this study. There are enough thought-provoking ideas in Langford's unblushingly British views to repay the attention of historians of eighteenth-century England, Europe, and America.

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STANLEY AYLING. *The Elder Pitt, Earl of Chatham*. New York: David McKay Company. 1976. Pp. 478. \$14.95.

Pitt was an enigma to his contemporaries, even to those who admired and followed him. Followers

were what his character required, for he had few of the traits that would have made him a good colleague. Aloof and deliberately intent upon giving the impression that he was above intrigue, Pitt had the aristocratic amateurs of his day at a distinct disadvantage. There was a large portion of the artful demagogue in his makeup and in his tactics. His methods are perhaps more easily understood today in an age of mass politics than they were two centuries or even fifty years ago. Rosebery decided in 1910 that the "opaque fog of mystery" that surrounded Pitt's life made the task of writing a biography an impossible one; he ended his attempt after five hundred pages and at the point where Pitt reached high office. Three years later, Basil Williams wrote the book that remains the most authoritative biography about Pitt. It is an imperial book that anticipates the great British Empire that was to follow.

Ayling's book is a biography of Pitt and a work of rare distinction. He has given us a witty, scholarly, and delightfully constructed book that reflects wide reading, mellow perception, and the effortless clarity that befits a man who has behind him a long career as a schoolmaster. Ayling has accomplished what few professional historians have the inclination or the ability to achieve: a leisurely assimilation of the monographs, articles, and documents combined with a worldly and tolerant perception of the quick turns of ambitious men. It is a post-imperial work written by a man old enough to have first studied Pitt as the progenitor of the British Empire.

Pitt was a ruthless partisan who ran with the pack that surrounded Cobham at Stowe. Walpole's draconian revenge on the opponents of the Excise Bill gave the first lord's opponents their opportunity for a coherent and lasting opposition to his ministry. Pitt's eloquence carried him early to the front ranks of the attack. His responsibility for Britain's victory in the Seven Years War is the one unequivocal achievement of his long and stormy career. He was thereafter, as John Brooke has described him after the resignation of 1761, too considerable to be neglected and too uncooperative to be assimilated. He threw away opportunities because he was essentially unable to play the game according to the rubrics of the eighteenth-century constitution. His personality was a pastiche of pride, patriotic rhetoric, swagger, ambition and was permeated by a condition that today would be called manic-depressive. He was, notwithstanding, the greatest British statesman of the eighteenth century and it is the finest achievement of Ayling that the reader is led to that conclusion about Pitt, warts and all.

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JOSEPH HAMBURGER. *Macaulay and the Whig Tradition*. Chicago: University of Chicago Press. 1976. Pp. xiii, 274. \$17.50.

The central theme of this carefully researched and closely argued book is what might be called "the primacy of trimming" in Macaulay's political thought and action. According to Joseph Hamburger, Macaulay is best understood as belonging to a tradition that includes Halifax and Burke, the trimmer tradition that emphasizes "the cooling of passions and the relief of grievances in order to reduce the risks of violent conflict" (p. 93). Commitment to this approach to politics can be found as early as Macaulay's Cambridge prize essay on William III written in 1822 and is consistent throughout his life and work. Hamburger stresses the fact that Macaulay's acceptance of the trimmer point of view rested upon a historical theory which held that alternations between anarchy and despotism had been, and would continue to be, a threat to European societies. The situation in England from 1815 to 1832, and the perennial political instability in France, provided evidence that the trimmer's stance was the proper one if the danger of revolution and civil war was to be averted.

In successive chapters Hamburger shows how this position found expression in Macaulay's politics and in his writing of history. It led to a willingness to undertake political reform, not necessarily with the aim of achieving some positive good but in order to preserve the fundamental institutions of society from attack and possible destruction. He condemned as doctrinaire both Eldonian diehards and Utilitarians: the former were prevented from practicing what Hamburger calls "conciliatory politics" by too great reverence for the past while the latter, at least in politics, were prone to extremism because of their tendency to think in terms of deductive principles instead of political realities. For Macaulay, it was history rather than abstract reasoning that could teach the statesman those lessons in "civil prudence" that should guide political action. His *History of England*, states Hamburger, can be distinguished from the Whig histories of Hallam and Mackintosh precisely because of its emphasis upon, and praise of, trimming. In his penultimate chapter Hamburger argues that Macaulay helped to shape the nineteenth-century Whig tradition by providing a principled justification for the trimming element which, in practice if not in theory, was one aspect of Whiggism itself.

Hamburger raises important questions both about Macaulay's political views and about the proper definition of the Whig tradition. His aim is explicitly revisionist, and certainly it is no longer possible, as a result of this book and the recent biography by John Clive, to accept the conventional picture of Macaulay as a smug Victorian

Dr. Pangloss. Yet one may wonder whether Hamburger has not over-systematized Macaulay's politics in accordance with the trimmer model and thereby ejected those tendencies and attitudes that are usually termed "Commonwealthman," "Foxite," or "liberal." Ultimately, his argument rests upon an interpretation of the Whig tradition, and its relationship to trimming, that some scholars may not find convincing. In any case, we are in debt to Hamburger for a lucid and challenging study which must be read and pondered by those interested in Macaulay, in British historiography, or in nineteenth-century political speculation.

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ABRAHAM D. KRIEGL, editor. *The Holland House Diaries, 1831-1840: The Diary of Henry Richard Vassall Fox, Third Lord Holland*. Boston: Routledge and Kegan Paul. 1977. Pp. lxiv, 513. \$31.25.

This is a most welcome addition to the printed primary sources for this period. Those familiar with Lord Holland's *Memoirs* and *Further Memoirs of the Whig Party* will not be surprised to find him in the role of a shrewd and urbane diarist. Abraham D. Kriegel has done an admirable job of editing and annotation and has supplied a most valuable introduction, "Lord Holland, the Whigs, and the Decade of Reform."

When the Whigs returned to power in 1830 Lord Holland, in constant pain from gout, declined the Foreign Office and became instead Chancellor of the Duchy of Lancaster, a sinecure but one that carried cabinet rank with it. As a result his diary contains accounts of cabinet deliberations and procedures. For that reason alone it must interest any modern British historian concerned with constitutional development. But, of course, the principal matter of substance before the cabinet in the early thirties was parliamentary reform, and the diary offers a fascinating day-to-day account of the ups and downs of the Whig cause. The extent to which various members of the Grey cabinet feared revolution, their (and the king's) shifting attitudes toward the creation of peers; their assumptions about the linkage of parliamentary representation to various economic and social interests—all these matters are amply and illuminatingly dealt with in Lord Holland's diary. The other chief political interest of the diary lies in foreign affairs, a subject to which the diarist was knowledgeably devoted. His own principal aim and desire in this field during the decade covered by the diary was for a Franco-British constitutionalist alliance against the despotism of the Holy Alliance. Since he showed himself indiscreet on occasion, he may not always have been made privy to all secret negotia-

tions. But this did not stop him from recording a good deal of news and gossip.

Lord Holland was not one of the great diarists. Cynicism, irony, and learning he possessed in plenty, but he lacked the combination of sparkle and ingenuousness required by that particular art form. Yet it isn't all suet pudding. There are enough plums to make perusal of the diary worthwhile, even for the non-specialist historian. About Brougham, October 1831: "The Chancellor in the course of his speech drank at least a bottle and half of mulled port and, taking some more after he had returned to the Woolsack, was so intoxicated that he could hardly put the question." About the Duke of Wellington, August 1833: "A more inveterate worshipper of power and a more bigotted enemy of the fundamental principles of political liberty certainly never existed. His theory of human society is truly that of an army." About Queen Victoria, August 1837: "Her family complexion and blood, her period of life and her inordinate love of *music*, all in his [Melbourne's] judgement indicate the germ of a warm constitution, and she has too much sagacity and intractability in her character not to ascertain that she is under no restraint but such as her own sense of prudence and propriety may impose."

But even more interesting than Holland's comments about (or quotations from) others are the glimpses we get into his own mind, character, and opinions. Thus his comment on Lord Durham's "childish" desire for an earldom: "Pauvre humanité." And this observation after William Cobbett's death in 1835: "The pride or the virtue of the Whig Aristocracy disdained in 1806 or in 1807 to admit a person of so low an origin, of such instability in principle, and of such scurrility in controversy to their councils or their Society. Had they prudently acted otherwise, Cobbett would have probably felt the value of a decently honest Character and laboured to preserve it. As it was he despaired of obtaining countenance or equality; he became desperate. He cast away all shame and was restrained by no scruple." The diary of a Whig statesman who was capable of setting down such observations is well worth reading. We must be grateful to Krieger for having made that possible.

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E. R. NORMAN. *Church and Society in England, 1770-1970: A Historical Study*. New York: Oxford University Press. 1976. Pp. 507. \$33.00.

Employing the full range of the arts of irony, the distinctive talent of the conservative historian, Edward R. Norman has written a close, interpretative survey of the social thought of the leaders

of the Church of England over the past two hundred years. The size and solidity of his achievement are impressive. On the eighteenth and nineteenth centuries, he has made full use of the considerable volume of research and analysis published since E. R. Wickham's pathbreaking study of Sheffield, *Church and People in an Industrial City* (1957); Norman goes on to examine for himself the printed sources on which this secondary literature is based. On the twentieth century, to which Norman devotes half of his book, the dearth of secondary scholarship obliges him often to conduct an original analysis of the printed sources and to dip into some collections of private papers. Much of what he has to relate, particularly on the years since World War I, will be unfamiliar to everyone but ecclesiastical insiders.

The interpretation which Norman develops through his survey is as striking as the massiveness of his research. He works his way with quiet authority through the eighteenth and nineteenth centuries to develop a thesis which he then aggressively applies to the twentieth century. The question which has riveted the attention of most commentators on Church and society in modern English history is why the Church (that is to say, usually, the Church of England) failed, in spite of impressive efforts, to win the support of the working classes and, much later, to hold the support of the middle classes. Norman discovers the model upon which he bases his answer to this question in the intellectually progressive clergy of the early nineteenth century, many of them soon to become bishops (see R. A. Soloway, *Prelates and People: Ecclesiastical Social Thought in England, 1783-1852* [1969]), who embraced the economic and social philosophy of Political Economy. They are familiar whipping boys. Later generations of Church leaders, appalled by the confined view of God and man which seemed to underlie such a philosophy, blamed the failure of the Church upon that pre-Victorian generation, whose sins they tended to see as perpetuated throughout the nineteenth and into the twentieth centuries. Norman reinterprets that model and then applies it to its castigators. He argues that those early nineteenth-century progressives illustrate "a constant theme in the social attitudes of the Church of England—the adoption, in almost every generation, of diluted versions of the most progressive ideas available, by that section of the leadership most in touch with academic idealism. . . . Church leadership has often been ineffective not because it has lagged behind the progressive intelligence of each age, . . . but because it has been too much in advance, too academic, too removed from the practical assumptions of ordinary men" (p. 42).

This pattern applies, as Norman would have it,

not only to the first half of the nineteenth century, when a perpetuation of paternalistic attitudes would have won a more appreciative response among the rural and urban working classes, but also to the last twenty years of the century, by which time Gladstonian views about the proper relationship between the state and the economy had been absorbed by the working classes, including the early Labour movement, only to coincide with the adoption by the most influential bishops of collectivist criticisms of *laissez faire*. The collectivist ideals preached by the Christian Social Union captured virtually the entire leadership of the Church in the first years of the new century. Although, by Norman's own account, similar assumptions began to take hold of the working classes, still the clerical leaders who enjoyed the greatest popularity among laymen—Henson, Headlam, and Inge—were the handful who stood out against C.S.U. orthodoxy.

The only decades when the leadership of the Church deviated from this pattern were the 1940s and 50s. The nightmare on the Continent, total war, and then the grip of Cold War, impressed the Church's leaders "with the truth that politics was about real power and actual coercion, and not merely the adoption of ideas"; while the prewar depression taught them "that economic and social questions were too technical and too contingent upon political action, to be of easy Christian solution" (p. 364). Sobriety did not last long. After 1960 the tension between conservative laity and radical leaders grew so acute that observers spoke of "two Christianities," one side holding steadfastly to familiar theological teaching, moral order, and traditionally conceived authorities; the other adopting religionless theology, deregulated morality, and the political philosophy of liberation movements in the Third World, whose concerns were still not the same as those of working-class socialists—status, wages, and the distribution of wealth.

The root of the Church's failure in Norman's eyes goes beyond philosophical trendiness to class. With very few exceptions, the clergy throughout the years covered by his survey have been middle to upper class by birth and education. Totally unfamiliar with the world beneath them, the clergy failed to recognize the moral values of the working classes and the "sturdy and inarticulate culture of religious belief" (p. 125) which they possessed at least in the nineteenth century. Disturbed, nevertheless, by their failure to reach over the gulf, the intellectually lively and hence often pace-setting clergy embraced an idealized, academic version of the advanced social notions available within their contemporary intellectual community. Class contaminated the enterprise from start to finish. Working men were rarely fooled by

it, and the middle-class laity were increasingly alienated.

No clergyman in Norman's account entirely escapes beyond the imprisoning walls of class. But Hewlett Johnson, the "Red Dean" of Canterbury, followed his socialist line of thought with a rigorous logic which Norman admires. The closest the book has to a hero is Hensley Henson, the Bishop of Durham, whose Nonconformist and poor, though still middle-class, background enabled him to appreciate the social values of ordinary people. But Norman cannot acquit even Henson of snobbery. In this powerful if idiosyncratic survey, class constitutes a large part of the embodiment in modern English society of Original Sin, the sin of which Norman as conservative and clergyman never loses sight.

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JAMES OBELKEVICH. *Religion and Rural Society: South Lindsey, 1825-1875*. Oxford: Oxford University Press. 1976. Pp. x, 353. \$25.25.

This useful study, which had its origins in a Columbia University Ph.D. dissertation, will be warmly welcomed by social and ecclesiastical historians. Unlike some monographs with wide-ranging titles, *Religion and Rural Society* really does address itself to the larger social issues of early Victorian religion. The author's aim is to describe the changing social structure of agrarian society and to relate religious phenomena (popular beliefs as well as religious institutions) to these social changes. For this purpose James Obelkevich has selected a relatively little-known area of the great rural county of Lincolnshire—south Lindsey, stretching from Gainsborough in the west to Mablethorpe on the coast and as far south as the river Witham. As this is a study of rural religion, the emphasis is on the villages rather than the market towns, though reference is made to the latter since they were inter-related with the surrounding villages. "The religious life of south Lindsey cannot be 'reduced' to its social foundations," argues Obelkevich, "but it is unintelligible without them" (p. 313). He shows how the transformation of traditional agrarian society into a class society transformed religion as well.

The first third of the book consists of an excellent survey of agrarian society in the middle years of the nineteenth century. If there are few surprises in this picture of the gentry, farmers, and laborers, it is extremely useful to have the documentation for it so clearly set out—especially on laboring life. Then follow three chapters on the Church of England, Wesleyan Methodism, and Primitive Methodism. A further chapter deals with

popular religion, defined as "the noninstitutional religious beliefs and practices, including unorthodox conceptions of Christian doctrine and ritual, prevalent in the lower ranks of rural society" (p. 261). In a final chapter Obelkevich draws together his conclusions and hazards some interesting remarks about specific social changes which accompanied the emergence of a class society and the ways in which the churches responded.

The plight of the Church of England, when to be "religious" meant to be Methodist, is amply brought out; and the Anglican response after 1860 (a Counter Reformation against the Methodist Reformation) is detailed in individual parishes. Obelkevich perceptively suggests that the great strength of Methodism—for farmers, shopkeepers, artisans, and laborers alike—was that it constituted a community within the dissolving wider community. The "means of grace" implied that the "means" above all served the personal needs of the participants. But as a contemporary asked, "when did you ever hear a man cry out for mercy in a church?" (p. 145). Methodism had its maximum appeal in the period of transition between community and class: "the Methodist class denied, temporarily at least, social class" (p. 217).

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DONALD J. OLSEN. *The Growth of Victorian London*. New York: Holmes and Meier. 1976. Pp. 384. \$22.50.

This book is a significant extension of Donald Olsen's *Town Planning in London: The Eighteenth and Nineteenth Centuries* (1964). It takes the story from two essentially Georgian and mid-Victorian estates in Westminster through the expanding suburbs to the edge of Greater London and the beginning of the twentieth century. More importantly, it moves through the history of architecture and townscape into social and cultural history, using the range of styles, the types of building, and the way in which old neighborhoods changed and new ones evolved to comment broadly on the Victorian way of life.

Olsen begins with the accommodation of new populations; the break-up of areas which combined office, shop, small manufactory, and home, and in which the rich, middling, and poor lived in adjoining streets; the new segregation by purpose and by class; and the introduction of specialized building—club, office block, hotel, department store. In a typical comment, he notes the reluctance to move into the new apartments, citing English pride of house and home and the horror of meeting the wrong sort of person on the stairs, in contrast to the decadent, flat-dwelling Parisians

with their enfeebled sense of family, privacy, and general morality.

Two of Olsen's major chapters are intriguingly entitled "The Rejection and Destruction of Georgian London" and "The Preservation and Extension of Georgian London." He devotes about half the book to what the Victorians inherited and what they did with it. Olsen stresses Victorian distaste for the Georgian facade and for the falsity of terraced houses disguised as palaces. He writes of the need for new buildings, new streets, new districts and of the propensity for contrast, variety, and individuality. But, despite esthetic and moral hostility to the Georgian mode, the managers of the great leasehold estates in Bloomsbury and Mayfair and the developers and speculative builders of the new suburbs of Bayswater, Belgravia, Pimlico, and Kensington continued the emphasis on squares and stuccoed, porticoed terraces, on building and planning that was recognizably of the older style.

There is an extensive and original chapter on the villa suburbs of outer London, others on the special problems of working-class housing and on the importance of the railways and other forms of transportation. The illustrations are excellent, though the maps are difficult to decipher. Moreover, Olsen is forced to depend on precise topographical references, and the reader unfamiliar with London place and street names will be to that extent in the dark.

This book is more than a monograph. Its liveliness and persuasiveness are based on two qualities: first, Olsen's love for London, his idiosyncratic taste for British conventions, his determined nostalgia for Victorian variety and eccentricity; second, a use of quotation so careful and extensive that one feels that Olsen was somehow an eyewitness of the growth he records.

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GILLIAN DARLEY. *Villages of Vision*. New York: Universe Books. 1976. Pp. viii, 152. \$16.00.

This handsome volume describes more than two centuries of efforts to deny the logic of modernization, industrialization, and urbanization by artificially recreating an imaginary rural past. Gillian Darley examines planned communities of such disparate nature as Blaise Hamlet and Saltaire, Edensor and Port Sunlight, New Lanark and Hampstead Garden Suburb, along with many less familiar attempts to use controlled residential environments to promote social, moral, and esthetic virtue. Whether designed for agricultural laborers

or industrial workers, middle-class commuters or religious zealots, they usually reflected some aspect of the paternalistic impulse, and the conviction that it is the home and its immediate surroundings that determine the manners and morals of society.

The author skillfully combines the techniques of architectural and social history, just as her villages combined the esthetic values of the picturesque with the moral and social imperatives of philanthropy. Evidence from printed and manuscript sources complements that of the actual villages, many of which are remarkably intact. Splendid photographs contribute to the argument of the text. A gazetteer listing and describing nearly five hundred planned villages in England, Wales, Scotland, and Ireland supplements Darley's analyses.

In the eighteenth century cultivated amateurs gave architectural expression to their eccentricities, both by creating new villages on their estates and by embellishing existing ones. The cottage and the village alike became deliberate esthetic creations. Early attempts to bring urban standards of uniformity and symmetry to the countryside were succeeded by equally artificial imitations of the irregularity and haphazard quality of the naturally evolved village. In the nineteenth century philanthropic and sanitary considerations grew increasingly important. At the same time many saw the village as a universally applicable solution to problems of urban growth, as reformers and speculative developers alike strove to bring its outward appearance, and sometimes its underlying structure, to suburban situations.

If the ideal of the village as the normal environment for society at large proved impossible to realize, the village as the chosen environment for groups of like-minded individuals—whether socialists, Chartists, Moravians, or, more commonly, artists and intellectuals—has been more successful. In identifying, cataloguing, and evaluating such pockets of resistance to the forces of concentration and conformity that were transforming modern Britain, Darley has demonstrated the role that utopian aspirations have played in modifying both rural and suburban realities.

DONALD J. OLSEN
Vassar College

H. E. MELLER. *Leisure and the Changing City, 1870-1914*. Boston: Routledge and Kegan Paul, 1976. Pp. ix, 308. \$18.75.

This book, based on a Ph.D. dissertation at the University of Bristol, is concerned with the development of leisure and cultural facilities within Bristol, primarily between the years 1870 and 1914. H. E. Meller chose Bristol for her study solely

because of its size and not because its evolution was typical of the times. On the contrary, as the book makes clear, the port was unique in its development in many areas, including its recreational facilities. Industrialization did not bring to Bristol the sharp changes experienced by other towns. The population of this port, for example, did not increase as rapidly as did that of other major industrial towns. Also, unlike other cities, Bristol already had a large middle class established in its own suburban town by the early nineteenth century, long before other growing urban centers had such satellite communities. This middle class had traditionally tended to dominate the cultural and social life of Bristol.

Many of the attempts to provide recreational and leisure facilities in the port were local in origin and execution—there was little integration into the larger national movements that had developed to promote leisure-time activities. Such wider connections appear to have been limited almost entirely to the meetings of the British Association held in Bristol in 1836, 1875, and 1898. Consequently, this book is valuable primarily for the information it imparts about Bristol alone during the given years and not as a study of a town undergoing a growth pattern applicable to other cities.

In Bristol there also does not seem to have been much working-class self-help activity, so common in many industrial towns. In fact, little is said about the lower classes except as recipients of the charitable and civic works of the middle class. However, the author does acknowledge that the lower classes devoted much time and energy to self-entertainment, unrecorded and therefore lost to the social historian. Like many of their brethren in other urban areas, the nineteenth-century working classes of Bristol spent a great deal of their recreational time and resources on activities condemned by the middle class—drink shops, street activities, and a few commercial entertainments. Meller does point out that changes were coming. The rapid development of organized sports and music halls showed their popularity among the working classes. Furthermore, the rise of the labor movement and the spread of the cooperative movement to Bristol in the late nineteenth century also had a growing influence on the organized leisure time activities of the lower classes.

LILIAN LEWIS SHIMAN
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DEREK FRASER. *Urban Politics in Victorian England: The Structure of Politics in Victorian Cities*. Leicester: Leicester University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1976. Pp. 324. \$24.00.

This book provides a compact survey of four aspects of urban politics in England in the second and third quarters of the nineteenth century: parochial and township administration, municipal government, parliamentary elections, and political agitation. While concentrating on Birmingham, Leeds, Liverpool, and Manchester, Derek Fraser also introduces supplementary material from half a dozen other cities. He uses apt quotations from such manuscript collections as the papers of George Melly, George Wilson, and Edward Baines, Jr. In addition to his own extensive research in the primary sources, Fraser draws on the many useful theses being produced by young historians.

The dust jacket reproduces a cartoon from the *Northern Star*, depicting a meeting of the "Fox and Goose Club" in Leeds in 1841—the middle-class foxes are about to hoodwink the working-class geese. The cartoon reflects the mood of Chartist militants who were contemptuous of the "wheedling twaddle" being dished out to the "workies." In order to understand the incident, however, the reader will have to turn to the chapter on Leeds in Asa Briggs' *Chartist Studies*. Although Fraser clearly has the knowledge to provide a full analysis of such events, he is unable to do so within the organizational framework he has chosen. Having divided his subject into twelve topical chapters, each of which is further subdivided so as to treat individual towns, he ends up serving rather thin slices. Thus, his account of the struggle over church rates in Birmingham in the 1830s fails to convey the intense social and religious conflict underlying the issue. In the chapter on the free trade movement, there is little trace of what G. K. Clark called "the festering bitterness of one class at another whom they believe to be insolently privileged at their expense." The Namierite phrase, "structure of politics," may indicate an inclination, as a matter of methodological principle, to empty political activity of its ideological content. Having relegated ideology to a relatively subordinate position, however, Fraser has not allowed himself enough space to set politics in a precisely defined social setting.

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THOMAS WILLIAM HEYCK. *The Dimensions of British Radicalism: The Case of Ireland, 1874-95*. Urbana: University of Illinois Press. 1974. Pp. xi, 297. \$10.00.

In this stimulating book T. W. Heyck examines late Victorian Radicalism and its relation to the Irish Question. He shows that the forces of British

democracy and Irish self-government seldom marched hand in hand. Oblivious to Home Rule arguments until Parnell made out a bare-knuckles case, many Radicals became Home Rulers in 1886 as a way of clearing the Irish from the House of Commons and leaving the Liberal Party to get on with its own more important concerns. Of the 160 or so Radicals elected in 1885, about eighty percent voted for the Home Rule bill, despite Chamberlain's defection. Home Rule was thus hardly the death knell of Radicalism heard by modern Chamberlainites like D. A. Hamer and R. T. Shannon. Quite to the contrary it left the Radicals as the largest group remaining within the Liberal Party, which was then radicalized over the ensuing years of opposition.

Heyck's work is filled with excellent commentary about the Anglo-centric, middle-class mentality of Radicalism, as revealed through the mirror of the Irish Question. But little attempt is made to reverse the image and use Radical views to reappraise the Irish dissidents, who are assumed to represent "a plainly outraged and suffering people" (p. 234). Parnell's quasi-legal agitation and Bonapartist hauteur are treated with an indulgence totally denied to Chamberlain's "authoritarian personality" (p. 148).

Of wider interest to historians will be Heyck's quantitative method. By multiple discriminant analysis of selected divisions in the House of Commons, he identifies Radical Members of Parliament between 1874 and 1895. This advance beyond impressionistic accounts of Radical strength is certainly to be commended. But one wonders why the Radical numbers here are considerably higher than contemporary estimates. Part of the discrepancy arises from the selection of too many Non-conformist test issues, along with a heavy dose of Labouchère's eccentric republicanism. Cannot this particular quantitative method be used to test other issues and thus discern a smaller but more meaningful Radical group? One might discover, for instance, how many of these Radicals favored various aspects of Chamberlain's socially aggressive Radical Programme and how many, like John Bright, Chamberlain's fellow "Radical" from Birmingham, worried about the Programme's "new fangled propositions." Michael Barker's *Gladstone and Radicalism: The Reconstruction of Liberal Policy in Britain 1885-1894* (1973) emphasizes the development of Radicalism, whereas Heyck sees the Radicalism of the 1890s as little different in ideology from twenty years before, despite some of his own non-quantitative evidence (pp. 167-77). Nevertheless, Heyck's placement of obscure M.P.'s in the Radical camp, despite problems of definition, will be helpful to many historians of the period.

MATTHEW R. TEMMEL
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JOE D. BURCHFIELD. *Lord Kelvin and the Age of the Earth*. New York: Science History Publications. 1975. Pp. xii, 260. Cloth \$15.00, paper \$6.95.

It is not easy to present the history of science with thoroughness so that it can also be grasped as a manageable intellectual problem by the regular historian. The great classical physicists have proven especially resistant to such efforts. By examining one stage of the debates on a topic over which several sciences came into conflict, a subject important for general cosmological thought, Joe D. Burchfield gives us one valid method of solution.

The book does not change our ideas of the history of geology or physics, nor does the reader have an easy time of it intellectually. The author has chosen perhaps the most difficult, and probably the most confusing stage of the debate, a period when the debaters were technically well-equipped but overly naive in their assessment of the extent to which their methods could give final answers. Burchfield is protected by his choice of subject from telling a story of how the correct gradually triumphed over the incorrect.

Briefly, Kelvin, following his mentor William Hopkins in geophysics, used the "laws" of heat from the 1850s to assert that the age of the earth was quite small. Uniformitarian geologists, following their mentor Charles Lyell in dynamical geology, argued for a much longer time-span. So did Darwin (a Lyellian in part). Thomas Huxley felt he had to explain the problem away. Many others were involved, and an observation by the author about one of them illuminates the whole affair: "Perhaps the most remarkable thing about this display of confidence was that it occurred without the stimulus of any real advance in the relevant geological evidence" (p. 149). No one knew then about radioactive materials in the earth's crust, so the terms of the debate changed rather sharply after 1903.

Recent history of science seems determined to show, more than anything else, that scientific findings have never been objective, verifiable, reasonable, modest, or (some would say) true. Hence, an increasing interest in sociological determinants of scientific activity. Burchfield's method strikes more directly at scientific objectivity than that. He narrates the lack of restraint, arbitrary assumptions, quick simplifications, loose calculations, and tendencies to vast extrapolations which scientists of one general "community" used in fighting each other. I think that these *are* important methods for the advance of science; it may enlighten the reader to find that the great Lord Kelvin apparently thought so too.

SUSAN FAYE CANNON
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RODERICK FLOUD. *The British Machine Tool Industry, 1850-1914*. New York: Cambridge University Press. 1976. Pp. xiv, 217. \$14.95.

Debate about the origins of Britain's economic decline should never be quite the same now that Roderick Floud's new book has appeared. It will provide grist for many mills; for those who believe that British enterprise was already senescent in the late nineteenth century there is the evidence of quaintly archaic systems of cost accounting and management seemingly common even in "best practice" machine tool firms. But for those who reject such a pessimistic argument and prefer the traditional hammer-blows of the First World War, there is the bulk of Floud's carefully assembled data and his judiciously phrased conclusion that in this one basic industry there is no evidence of a failure of productivity or enterprise in the later nineteenth century. Floud himself is much too cautious to really take sides (indeed, he strikes me as having been bolder in his paper published in Donald McCloskey's *Essays on a Mature Economy: Britain after 1840* [1971]), but it is clear that his heart is not with the "pessimists." Most of all, however, he argues, quite rightly, that it is too easy to talk in aggregate generalities about the poor performance of the British economy without analyzing the actual nature of that performance at the level of individual industries. It is to the latter task that Floud has addressed himself, and, using the enviably complete records of Greenwood and Batley, he has both written a history of the machine tool trade and analyzed the market structure, productivity, and performance of this one firm.

The outstanding feature of the book is the ingenious and sophisticated methodologies employed to tease out of the data the answers to his major questions. There is a particularly interesting use, for example, of directories to explain the structure of the industry and to show how mistaken are those who believe British engineering of the period to have been conservatively resisting specialization. But the main body of his analysis is the complex econometric formulae which he skillfully uses to determine the productivity and efficiency record of the firm. What Floud has to say is important both in itself and for the way he arrives at his conclusions. My fear is that his case has been so cautiously presented and surrounded with such an impressive display of econometric skills that the book may not reach the wide readership of general historians that it deserves.

RICHARD N. PRICE
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W. R. P. GEORGE. *The Making of Lloyd George*. Hamden, Conn.: Archon Books. 1976. Pp. 184. \$12.50.

This unpretentious little book by the famous Welshman's nephew, a Criccieth solicitor and county councillor, is a valuable contribution to the multiplying Lloyd George literature. It draws upon the largely untapped family and business papers of the author's father, William George, Jr.—a formidable archive recording the activities of three generations of the George and Lloyd families since the early nineteenth century. The author quotes liberally from these documents, many of which were written in Welsh.

The William George papers have not been open to researchers. Recent critical studies of Lloyd George's early career, including the ambitious series begun by John Grigg, have suffered from the inaccessibility of his family archive.

Although George makes several astute observations about his uncle, this book does not furnish any striking disclosures, or even many new insights into the developing personality of the "Welsh wizard." Rather, its evidence reinforces what has become the standard portrait of an ego-centric, ambitious, and opportunistic young politician, driven by a powerful sense of mission to transform Welsh politics and make his mark on London as well. Lloyd George specialists may question the author's opinion that his uncle's religious beliefs were quite profound and his suggestion that Lloyd George at an early age had formed a deep concern for social problems. They will agree more readily with the author's reiterated contention that without the unselfish support of his hard-working, dependable brother, Lloyd George's political career could not have been launched successfully.

The documents cited by the author may be most helpful in illuminating the misty portrait of Lloyd George's schoolmaster father. Like his famous son, William George, Sr. was a hypochondriac, vain, attracted to women, prodigal with money, bourgeois in outlook, and addicted to self-pity. William George felt that he had lost status when he married Betsy Lloyd, a servant girl. We also learn that when he died, George was moderately affluent, with invested savings worth about \$20,000 in today's currency. These savings later paid for the legal education of his two sons and supported the Lloyd George family in relative comfort for many years. Readers are left to ponder what might have happened if William George, Sr., had migrated to New Zealand or purchased a bookstore with his life savings, both of which he contemplated after losing interest in school-teaching.

DON M. CREGIER
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This is the story of Nonconformist involvement in British politics in the decades before and after the First World War, the climax being the Liberal landslide of 1906 and the nadir being the failure of the Council of Action in 1935. Despite the title, most of the discussion concerns the twenty years between 1902 and 1922 and half is spent on the period before 1914. These years saw marked shifts in Nonconformist political allegiance and a decline in Nonconformist political influence. This emphasis provides an interesting perspective on the period, but Stephen Koss' treatment of it is disappointing.

First, there are too many mistakes. Some are just careless, like the mistake in the name of the United Reformed Church in the opening sentence. Others reveal a superficial knowledge of religious terms, e.g. the description of Bishop Henson as a high Anglican (p. 179) or the assertion that the Forward Movement was a reaction "against Calvinist orthodoxy" in Wesleyanism (p. 21)! More serious is the misquotation of Silas Hocking on page 33, where a reference to Baptist, rather than Wesleyan, hostility to the agitation against the Boer War destroys both the sense of the quotation and the point being made. Such mistakes do not inspire confidence in the author's scholarship.

Second, there are omissions in the analysis. It is surprising to find no consideration of the impact of the German invasion of Belgium on Nonconformist attitudes to the outbreak of war in 1914. Nonconformists are blamed both for their bellicosity in 1914 and their weakness over appeasement in the 1930s; but Koss does not consider the extent to which both attitudes were characteristic of British feeling as a whole at the time, nor does he take the opportunity of presenting a thorough discussion of the significance and extent of pacifism.

Last, though the book makes it clear that the alliance between Liberalism and Nonconformity began to break up in 1910, no explanation for the shift of Nonconformist political opinion at this time is offered. Here Koss' excellent discussion of changing attitudes in the political leadership may have put the emphasis in the wrong place. The possibility that the resurgence of political Nonconformity between 1902 and 1910 was an exceptional episode needs more consideration, as does the distinction between religious issues in politics and the political interests of religious groups. There is much interesting information here, but not enough unhurried reflection.

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STEPHEN KOSS. *Nonconformity in Modern British Politics*. Hamden, Conn.: Archon Books. 1975. Pp. 263. \$14.50.

KEITH ROBBINS. *The Abolition of War: The "Peace Movement" in Britain, 1914-1919*. Cardiff: University of Wales. 1976. Pp. 255. £5.00.

Keith Robbins attempts to weave together the diverse strands of the British "peace movement" by examining the motivations and objectives of its disparate elements and assessing its impact on events both during the Great War and in the inter-war period. The results, though not without merit, are at best uneven.

By defining the "peace movement" broadly, the author is able to incorporate peace-by-negotiations groups and planners for a new world order in his study and, through them, to provide an interesting perspective on "respectable," non-pacifist alternatives to the official view that the war could only be ended by a military victory. Less original but also useful is his summary of the ideas and activities of the not-so-respectable Union of Democratic Control. When Robbins turns to more radical war resisters, however, he begins to falter, sometimes badly. His treatment of Bertrand Russell is a case in point.

To say, as Robbins does, that "the fastidiousness of his [Russell's] pacifism . . . typified the superior refinement of that Cambridge-Bloomsbury circle to which he belonged" (p. 88) is akin to comparing a bloodied veteran of the trenches to a group of home front jingo patriots. Although Robbins does admit to Russell's possessing "vestigial backbone" (p. 88), he consistently disparages not only Russell's philosophical position on the war but even the usefulness of his contribution to the movement. Materials in the Russell Archives, offering abundant evidence of the time, energy, and patience Russell poured into the peace effort, have led other scholars to very different conclusions. Robbins has used some of this material to good advantage, but the reasons for his peculiar interpretation of it are never clear.

In dealing with the No-Conscription Fellowship, Robbins rightly emphasizes the wrenching disputes which constantly troubled that organization, but he often confuses the nature of these quarrels and even the position of various disputants. In any case, he believes such factional struggles demonstrate that the leaders of the N.C.F. were as ignorant of the Fellowship's "real purpose" (p. 167) as other pacifists were of the meaning of "peace" (p. 122). It may be true that an excess of love or zeal or idealism bewildered many pacifists and foredoomed their movement to frustration and failure. But on the way to arriving at this conclusion, Robbins exhibits such a curious and sometimes heavy-handed malice toward war resisters that one wonders about his motives for undertaking the journey.

Some of the errors and distortions in the author's treatment of individuals as well as groups might have been prevented had he not ignored so much of the recent scholarly work on the peace movement. In fairness, it should be noted that the

printing of the book may have been long delayed since there is no bibliographical entry after 1972. While one can admire Robbins' determination to go to manuscript sources, it is impossible to accept the sort of interpretations which result from his not going far enough.

In sum, Robbins' work is highly readable and on occasion informative, but it would be unfortunate if anyone supposed that this was the last word on the British peace movement in the First World War.

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G. A. PHILLIPS. *The General Strike: The Politics of Industrial Conflict*. (Radical Men, Movements and Ideas.) New York: Holmes and Meier. 1976. Pp. xii, 388. \$19.50.

The British General Strike of 1926 remains, fifty years after the event, one of the most fascinating episodes in European labor history. For nine days, May 4-12, 1926, the largest and most powerful trade union movement in the world brought out approximately three million transport workers, printers, iron and steel workers, and building tradesmen in support of a national coal miners' strike, thereby shutting down virtually the whole of British industry. In response, Baldwin's Tory government mobilized troops, police, and special constables to maintain order and to secure the movement of essential supplies. Despite the potential for class war which this massive shut-down created, however, the stoppage was remarkably peaceful. Aside from some rioting in Newcastle and a number of clashes between picketers and the police, both government and local strike committees generally refrained from provocative action—a matter which called for special restraint on the part of the workers after the disastrous settlement in which the Trades Union Congress called off the strike without gaining any of the miners' objectives.

G. A. Phillips has written what is by far the most scholarly and authoritative of the several new accounts which have been published to commemorate the strike's golden anniversary. He correctly attributes the lack of revolutionary zeal to the fact that large-scale British strikes, unlike those in France, have never had a political purpose. Instead, the author argues, the General Strike grew logically out of the increasing scale of unionism which had developed in Britain since the 1880s, particularly among unskilled and unapprenticed workers. This argument is not wholly original, being implicit in several other accounts of the strike. Nevertheless, despite his somewhat heavy prose, Phillips develops it with considerable ele-

gance and care, making excellent use of primary sources from a wide variety of collections. The argument's weakness lies in exaggerating the role of organizational growth, as such, in bringing about the General Strike; and in derogating other factors—such as miscalculation, fear of humiliation like that which occurred on Black Friday in another dispute in 1921, or stubbornness on the part of the government—of which Phillips is well aware, but which he chooses to play down in his analysis for no good reason. Also missing from the book is any sense of drama, or of the crisis atmosphere which swept the country during that fateful week; still less is there any sustained attempt to get inside the thoughts and actions of the strikers themselves. This is an organizational, London-oriented study; but, within these limits, an excellent book nonetheless.

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DAVID HOWELL. *British Social Democracy: A Study in Development and Decay*. New York: St. Martin's Press. 1976. Pp. 320. \$16.95.

Labour Party history since the debacle of 1931 has been largely the province of journalists. While there are useful studies of the party's foreign policy by John Naylor and Matthew Fitzsimons, there has been little systematic investigation of Labour's domestic policy. The growing volume of memoirs and the availability of the minutes of the National Executive Committee under a fifteen-year rule now provide a basis for such studies, and David Howell's work provides a promising beginning.

The decade of the 1930s, generally regarded as the nadir of the party's history, is viewed by Howell as a creative period when Labour produced what it had lacked before 1931 and what has been absent since 1951, a concrete program consistent with its traditional socialist vision on which the various elements in the party could agree. After the postwar Labour government faithfully carried out the agenda of ambitious social welfare schemes and limited nationalization the party was unable to formulate a new policy compatible with its ideals of fellowship and collectivism which would have sufficient electoral appeal. The prosperity of the 1950s and the economic deterioration of the 1960s and 70s have proved equally destructive of the party's socialist ideology. Affluence and the growing acceptance of Keynesian economics as a means of managing the economy made proposals for further nationalization seem unnecessary, and the party moved toward the acceptance of the existing combination of capitalism and public ownership. The nation's declining economic posi-

tion has led Labour to policies designed to modernize British industry but which threaten the entrenched interests of the trade unions that form the basis of the party. Labour governments now attempt to prop up, with measures scarcely distinguishable from those of the Conservatives, the faltering capitalist economy the party once pledged to transform radically.

Howell traces these developments in great detail. The result is a history of the Labour Party during the last four decades which is distinguished by the author's thoughtful judgments on men and events.

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LARS ÅKERBLÖM. *Sir Samuel Hoare och Etiopienkonflikten, 1935*. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 76.) Summary in English. Uppsala: Historiska institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell International, Stockholm. 1976. Pp. 229.

The Hoare-Laval Plan of December 8, 1935 has aroused controversy since its inception. In Britain it caused a public uproar and led to the resignation of Foreign Secretary Sir Samuel Hoare ten days later. Largely on the basis of the contemporary records of the British Foreign Office and the protocols and papers of the Baldwin cabinet, this dissertation seeks to determine precisely what political and strategic factors influenced British policy during the Ethiopian conflict up to the end of 1935; how that policy was formulated; whether Britain pursued it consistently in Geneva, Paris, and Rome; what took place in Paris in December; and how the notorious Plan was received by the British cabinet and by the public in Britain and the Dominions. The focus is on Hoare and his activities between June 7 and December 18. In a brief excursus the author also analyzes the impact of the Ethiopian conflict on the British parliamentary elections of November 1935.

Although the author finds that "the views propounded in published works on Hoare's diplomacy [are] open to substantial reinterpretation" (p. 198), most of the facts and views he offers are not novel; we have seen them scattered throughout the vast literature provoked by this affair. What Åkerblom has done is to separate fact from myth. On these tightly written pages, replete with 1,004 footnotes, the story of the Hoare-Laval Plan, its origins and its immediate repercussions, emerges. Hoare himself comes through not as the deceitful betrayer of the League, nor as the sick and exhausted victim of Laval's tricks, but as the consistent appeaser concerned about his country's secur-

ity, yet woefully inexperienced in foreign affairs and tactically inept as a politician.

It is unfortunate that this study is available only in Swedish and in the restricting format of a dissertation. A translation is very much in order.

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WILLIAM GREIG. *General Report on the Gosford Estates in County Armagh, 1821*. Introduction by F. M. L. THOMPSON and D. TIERNEY. Belfast: Her Majesty's Stationery Office, 1976. Pp. xi, 244. £4.50.

William Greig was a product of the Scottish Enlightenment as well as a practical man who brought its ideas and ideals quite literally down to earth. A versatile Lowlander of modest background and immodest aspirations, he was one of those indefatigable improvers who followed in the wake of such eminent agriculturalists as Marshall, Kent, Sinclair, and Young. Lacking formal education and connection, Greig nevertheless shared some of that splendid optimism about the ability of "science" to promote the well-being of mankind. Science meant to men of reason the testing and the reforming of all laws and institutions according to the universal standard of utility—as that term was defined by Bentham. For Greig science had the power to increase not only crop yields and rent rolls but also the sum of human happiness.

Greig began his career as a lowly road engineer in Scotland. Around 1808 he ventured to Ireland in search of further road work. An incurable autodidact, he taught himself the elements of civil engineering, surveying, geology, agronomy, and, of course, political economy. His articles on Irish agricultural economics (1816–17) led to his commission in 1818 to survey and analyze the county Armagh estate of the Earl of Gosford who apparently stood in need of some expert advice about ways to extract more rental from his estates. For three years Greig scrutinized and mapped every holding or farm on this 8,000-acre estate. With the ardor of a modern cliometrician he gathered all kinds of vital information about the 3,000 persons who tried to make a living on those lands. Greig finished *The General Report* in 1821. Describing himself rather quaintly on the title page as "Rural Designer, and Surveyor, in general," he supplemented the Report with many maps and elaborate tables (excluded from this edition) about the economic, social, and demographic structure of the estate. Greig packed his Report full of advice about ways to improve productivity and thereby to increase rental income in the long run. The Report also contains numerous "philosophical" asides, Malthusian homilies, and reflections about the

joys of rational farming. Besides enumerating the ingredients of scientific agriculture, Greig also urged Lord Gosford and his agent, William Blacker, to increase the size of farms, to draw up tighter leases, to forgive arrears, to design an efficient division of labor, and to find more thrifty and industrious tenants. Hardly any aspect of landlord-tenant relations escaped Greig's improving eye.

Greig fondly hoped that his Report would lead to a permanent position as resident improver or expert agriculturalist on the Gosford estate. But Lord Gosford had other plans or priorities. He sank much of his precious capital not into agricultural improvements but into a show-piece castle which cost some £80,000. Poor Greig received a pittance for all his efforts and wandered off into complete obscurity. When Greig alluded to the historical significance of his survey, he was not being entirely vain or foolish, as he left a rich legacy to Irish social and economic historians.

This edition of the Report is a fine example of what the Public Record Office of Northern Ireland has been doing for years to promote the recovery of Ireland's past. F. M. L. Thompson has contributed a thoughtful and informed introduction which places Greig and his Report within the context of British and Irish agriculture in the early nineteenth century. David Tierney has summarized the contents of the various tables in a lucid and helpful manner. The result of this combined operation between Belfast and London is not only a first-rate primary source for students of Irish land and society but also a reminder of how much can be achieved when enlightened archivists and historians decide to cooperate.

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LEON Ó BROIN. *Revolutionary Underground: The Story of the Irish Republican Brotherhood, 1858–1924*. Totowa, N.J.: Rowman and Littlefield, 1976. Pp. 245. \$13.50.

A number of historians have discussed highlights of the Irish Republican Brotherhood story, especially the 1860s and the 1905–22 period. In *Revolutionary Underground*, Leon Ó Broin presents a comprehensive portrait of the IRB from its beginning in 1858 as an effort to achieve a democratic Irish Republic through physical force to its end in 1924 when members of the Free State government decided that there was no room in an independent Irish nation for a secret revolutionary army. As in his previous books on Irish revolutionary nationalism, Ó Broin provides a British perspective from Dublin Castle source materials.

Ó Broin emphasizes how the IRB incorporated

revolutionary traditions of 1798 and 1848; the disgust of many Irish people with the self-seeking, unscrupulousness, and incompetence of many parliamentary nationalists; and the intense Anglo-phobia of Irish-America. The Fenian Brotherhood, the American wing of the IRB, gave the entire republican movement its popular romantic name. After 1870, the Clan na Gael, successor to the Fenian Brotherhood, kept the IRB alive in Ireland with money and prodding during a period when the organization was threatened by both apathy and schism. Ó Broin is particularly good when discussing the contest between the IRB and its rival, the Irish National Brotherhood, for control over the republican movement; the important role of the IRB in sustaining Parnellism after the fall and the death of the great leader; and its post-1900 resurgence as the decisive force in the Irish liberation effort. Michael Collins, leader of the IRB during the Anglo-Irish War, 1919–21, is Ó Broin's hero: "it is not unreasonable to conceive everything that went before as a preparation for him and his remarkable leadership. . . ."

Ó Broin should have paid more attention to the formative years of the IRB, and he perhaps exaggerates its influence in the Anglo-Irish war. But, all things considered, this is a valuable contribution to the literature of Irish nationalism. As usual, Ó Broin offers his readers interesting, well researched material in an organized, thoughtful, witty, and lucid style.

LAWRENCE J. MCCAFFREY
Loyola University of Chicago

ANAND C. CHITNIS. *The Scottish Enlightenment: A Social History*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 279. \$17.50.

From about 1750 to 1850—a crucial century of transformation which saw the creation of the first industrial society in Great Britain—philosophy, science, and social theory in the English-speaking world were dominated by Scottish thinkers. How did this small and relatively impoverished nation attain cultural hegemony, however transient, over its more prosperous and imperial southern partner? To a large extent Scottish philosophy was powerful because it incorporated new ideas that emerged from the intellectual ferment of Continental debates. For this reason, it is often more appropriate to speak of the Enlightenment in Scotland. Rather than merely collecting and repeating these ideas, however, the Scots recast them into doctrines and systems that addressed both the inherited concerns of their country and the larger interests of Great Britain. This process of interaction between ideas and social commitments fascinates intellectual historians and has stimulated the re-

cent revival of interest in what can be properly termed, from this point of view, the Scottish Enlightenment.

Chitnis offers his account of this subject not "as a definitive study" but as a summary of "the research of the 'sixties and early 'seventies." Rather than emphasizing politics, a minor theme in this movement, he focuses on the social organizations that encouraged Scottish thinkers to go beyond the commonplaces of an improving society. Thus his social history is basically a narrative description of those central Scottish institutions, the Church, the Law, and the Universities, in so far as they contributed to the Scottish Enlightenment. Chitnis, untroubled by a sustained concern with quantitative, theoretical, or comparative analysis, tells this story in a lively and appealing manner. Despite an inexcusable number of printer's errors, the book provides a convenient introduction to the institutional background that shaped and sustained Scottish thought.

Chitnis handles the more demanding task of unraveling the intricate relations between philosophical doctrines and social commitments, a secondary theme of this book, with much less success. Ideas are analyzed in a hurried and faulty manner, and their integration with the institutional narrative is superficial. But these deficiencies, while weaknesses in the book, also reveal the complexity and depth of its subject. So long as we remain curious about the connections between ideas and society, the Scottish Enlightenment will repay careful study.

ARTHUR DONOVAN
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CLAUDE LONGEON, *Une province française à la Renaissance: La vie intellectuelle en Forez au XVI^e siècle*. Saint-Étienne: Centre d'Études Foréziennes. 1975. Pp. 623.

This is a massive doctoral thesis written by a highly competent scholar, equipped with an essentially literary training and desirous of carrying out a work that would combine the studies of "history" and "literature" in the way that Lucien Febvre called for years ago. Claude Longeon's subject is the intellectual life of the province of Forez in the sixteenth century, a province or *pays* noted for having produced Honoré d'Urfé, author of the classic pastoral novel *L'Astrée*. And indeed it becomes clear that one of the main concerns of the author—his major question in fact—is to try to explain how this conservative, self-satisfied province, which, despite its geographic proximity to Lyons, experienced only at a distance for the most part the great movements and events that shook France in the sixteenth century, could have pro-

duced an author of genius such as Honoré d'Urfé. In this sense Longeon's concerns remain essentially literary.

The work is somewhat deceptively laid out, since it seems to offer much more about popular mentalities than it actually presents. In reality, it is virtually two separate and quite different books. One, the shorter, summarizes what is known about Forez in the sixteenth century and adds some new information, based upon archival research, about the social world of the writers, the tools they had to work with, and the educational institutions of the province. The second, much longer, is a relatively traditional but impressively thorough and highly competent study of the literature (all of it more or less mediocre) of sixteenth-century Forez before *L'Astrée*, including an analysis of the major influences on this literature, the different genres, and its major concerns. The social history in the early part is relatively rudimentary in method and conception, considering the quality of work now being done in the *Annales* school and elsewhere, but it is much more than simply routine "background" and reflects a substantial effort by the author to do real historical research. Longeon then tries to tie in this information with some interesting speculations as to why this particular province would become so important in the history of French literature. (For instance, and I oversimplify: because of the isolation of Forez from the main currents of thought and action, its economic stagnation, and its lack of possibilities for advancement for the intellectual elite, the myth of a golden past for the province becomes especially appealing and helps to stimulate the development of the pastoral novel.) But until we know other provinces as well as we now know Forez these will have to remain speculations.

Longeon deserves credit, then, for having made a beginning effort in the direction of combining the studies of history and literature as they are now being carried out in France. In the process he has increased our knowledge of the literary world of sixteenth-century Forez enormously.

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HARTMUT KRETZER. *Calvinismus und französische Monarchie im 17. Jahrhundert: Die politische Lehre der Akademien Sedan und Saumur, mit besonderer Berücksichtigung von Pierre Du Moulin, Moyse Amyraut und Pierre Jurieu.* (Historische Forschungen, number 8.) Berlin: Duncker and Humblot. 1975. Pp. 486.

Hartmut Kretzer has undertaken to examine the political teaching of the two leading Calvinist aca-

demies in seventeenth-century France, Sedan and Saumur. He proposes to show that Calvinist political theory does not represent an unbroken tradition of republicanism or tyrannicide. On the contrary, from the accession of Henri IV to the Revocation of the Edict of Nantes the most influential Calvinist professors in France defended absolute monarchy by divine right and taught the unconditional duty of secular obedience from all subjects, whatever their rank. Tangentially Kretzer also notes the absence of any Weberian capitalist or bourgeois consciousness in the political perceptions of his authors. Neither finding is novel.

Beyond the conventional rationale of adding to knowledge, Kretzer sees the justification of his work in the lesson that theologians ought to maintain an independent critical position in the face of government, and must not glorify Caesar at the expense of God. He believes this is precisely what the royalist French Calvinists did when they allowed fear of the Catholic majority and anxiety for their own institutional survival to shape their political thinking. In his view their defense of absolutism was not only theologically and morally irresponsible but also self-defeating: they contributed to the Revocation of the Edict of Nantes and left themselves without intellectual weapons when catastrophe came.

It is certainly possible to divorce political theory from its experiential matrix and to discuss the relationship between theology and politics *sub specie aeternitatis*. Such an approach, however, is apt to tell us more about the present than the past. Kretzer's argument, although philosophically and politically provocative, is fundamentally ahistorical.

Students of seventeenth-century political thought, or of Huguenot problems, will find his book interesting for its systematic and exhaustive documentation from the sources, which speak for themselves. There is an index of persons and a very extensive bibliography.

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ALBERT N. HAMSCHER. *The Parlement of Paris after the Fronde, 1653-1673.* Pittsburgh: University of Pittsburgh Press. 1976. Pp. xxii, 270. \$15.95.

Recent studies in English on the Parlement of Paris during the Old Regime include books by A. Lloyd Moote, J. H. Shennan, Christopher Stocker, and now Albert Hamscher, who has chosen to study the Parlement of Paris after the Fronde, specifically the years from 1653, when Mazarin returned to Paris from exile, until 1673, when the Parlement's right of remonstrance was reduced to a mere formality by Louis XIV. Hamscher exam-

ines how and to what extent the crown controlled the Parlement of Paris during these transition decades, never before studied in depth, in order to assess the achievements and limitations of royal absolutism.

Analyzing the Parlement's membership in the first two chapters, Hamscher concludes that the crown did not reform procedures of officeholding or change the court's composition: the crown did not attempt to control the Parlement by influencing its recruitment. Chapter three states that royal attacks on the sources of *parlementaire* wealth, which led to the Fronde, decreased after 1653. Chapters four and five, the heart of the book, are entitled respectively, "Parlement in Opposition, 1653-1660" and "Parlement in Submission, 1661-1673." Hamscher concludes that the Parlement's opposition in the 1650s was a legacy of the Fronde and a result of the crown's weakness. In this, he agrees with A. Lloyd Moote's characterization of the Fronde's legacy as a "victory-in-defeat," a learning experience in moderation for the crown. In fact, Hamscher's book is a sequel to Moote's study of the Parlement of Paris from 1643 to 1652. The Parlement was more submissive after 1660, but Louis XIV's control over the court was never absolute. During the king's personal reign, the Parlement of Paris was neutralized as an effective obstacle to royal policies when the king rectified the weaknesses of Mazarin's administration and curtailed its opposition by a "reformation of justice." The final chapter analyzes this "reformation" in which Louis XIV legislated modifications of the Parlement's powers and practices, particularly by the well-known ordinances of 1667 and 1670.

Clearly organized and well written, Hamscher's book is enjoyable reading, and for this reason it should be a useful study for teaching. In this, it is similar to J. H. Shennan's summary study of the Parlement of Paris during the Old Regime. Hamscher's work is based on original archival research, and it is therefore valuable to scholars. However, his documentation is skimpy at times, for instance in his quotation of office prices, and his utilization of sources is somewhat spotty. Having read François Bluche or Roland Mousnier, one sometimes has a sense of *déjà vu* in Hamscher's early chapters, while Louis XIV's "reformation of justice" has already been well studied. One also questions Hamscher's choice of such a restricted time period; perhaps the year 1683 or even later would have been a better stopping point. Nonetheless, his book is concise, workmanlike, and readable, a needed and informative study of a critical transition period in seventeenth-century royal government. It well merits the attention of historians of

the French parlements and the governments of Mazarin and Louis XIV.

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JOSEPH KLAITS. *Printed Propaganda under Louis XIV: Absolute Monarchy and Public Opinion*. Princeton: Princeton University Press. 1977. Pp. xiii, 341. \$19.50.

Printed propaganda" is an excellent theme for historical study, particularly in early modern Europe, where, thanks to the new art of printing, the literate public had begun to be inundated with thousands of cheap pamphlets, hardcover partisan histories, gazettes, and other news serials, not to mention routinely-published edicts, proclamations, and other decisions of state. Unfortunately, historians have usually dismissed that massive documentation as "worthless sources" or misused it to fill in the gaps of more "objective" sources (witness Grand-Mesnil's *Mazarin, la Fronde et la Presse, 1647-1649*). Recently William Church has pointed the way to a more imaginative use of these sources in his *Richelieu and Reason of State*. Now in Joseph Klaits' hands, the subject and subject matter of government printed propaganda have been fashioned into a fresh and provocative study of the early modern state in process.

Klaits has wisely concentrated on a peak period of propaganda, the War of Spanish Succession, and on the enthusiastic propaganda chief and foreign secretary to Louis XIV, Colbert de Torcy. Successive chapters deftly move from one type of printed source to another while simultaneously advancing the story chronologically from early war propaganda through the morale-bolstering publications of the bleak middle years to the carefully orchestrated peace offensive in print. Also provided are background sketches on Richelieu, Colbert, and the pre-Torcy years, plus a tantalizing, impressionistic introductory essay on propaganda in general.

Klaits convincingly shows that Torcy tried to influence "public opinion," among French subjects and the elites of England, the Netherlands, the Germanies, the Swiss cantons, and elsewhere, in favor of Louis XIV's war policies. Moreover, the propaganda writers drew on deep personal knowledge of various national legal-constitutional traditions and local authors like Grotius and Pufendorf in their attempt to convince foreigners that their countries' interests and traditions dictated an accommodation with Louis XIV. Though the author carefully avoids making exaggerated claims for the results, he does indicate a very sizable reading public for Torcy's propaganda.

It would take another book to develop the author's suggestive thesis on the assumptions underlying the medium and message of the propaganda he has so thoroughly discussed. Have there been two different views of propaganda, a traditional and a modern one? Did premoderns view knowledge as unitary, human nature as unchangeable, and propaganda as essentially underscoring old myths like the cult of sacred kingship? Do moderns, in contrast, agree with Locke that knowledge is relative and humans malleable, and hence decide to argue rationally and factually in the open arena about policies? And did Torcy—and even the Sun King—participate in the peak transition period between mythmaking and argumentation? It will be interesting to see these hypotheses tested in future studies of propaganda.

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KLAUS MALETTKE. *Opposition und Konspiration unter Ludwig XIV: Studien zu Kritik und Widerstand gegen System und Politik des französischen Königs während der ersten Hälfte seiner persönlichen Regierung*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 49.) Göttingen: Vandenhoeck and Ruprecht, 1976. Pp. 402.

This book corrects the widely held misapprehension that political criticism and military opposition disappeared in the first decades of Louis XIV's personal reign. Adducing evidence from archives in Holland, Belgium, Austria, and Spain, Klaus Malettke describes how from 1674 to 1678 nobles in Normandy, Guyenne, Languedoc, Dauphiné, Provence, and Roussillon sought, through diplomatic activity abroad and by agitation at home, to coordinate armed uprisings with arrivals of foreign invasion forces. The conspirators wished to found independent republics, prospective new polities for which they formulated interesting constitutional proposals. Malettke's impressive documentation makes untenable recent statements such as, "during the first generation of Louis XIV's personal reign the voices of criticism fell silent . . ." (W. F. Church, *Richelieu and Reason of State*, p. 511).

Most fascinating is a project presented to William III for a *Corps de République*. The lands it encompassed—Dauphiné, Languedoc (including Vivarais, Rouergue, Foix, and Cevennes), Guyenne (including Perigord), Saintonge, and Poitou—are virtually identical with the territories of the sixteenth-century Huguenot federal republic. Unfortunately Malettke points to this striking correspondence without comparing the conspirators' project with the second part of the *Reveille Matin*.

For it influenced directly the forty articles propounded by delegates meeting at Millau in 1573 (reprinted in Haag, *La France protestante*, 1846–58, volume 10, pp. 116–27)—a document important for establishing the Huguenot republic (see J. H. M. Salmon, *Society in Crisis*, pp. 189–93).

Did discovery of these preparations for secession influence the crown's decision to revoke the Edict of Nantes? Did the conspirators speak for some sections of Huguenot opinion? Malettke does not raise these perhaps unanswerable questions. But surely Louis XIV's ministers may have doubted the popular loyalty of regions where aristocratic revolt appeared amid frequent grass-roots rebellion. Indeed, it was in the heat of noble, peasant, and plebeian uprisings in June 1674—when, Malettke shows, the crown was most fearful of foreign intervention—that Colbert wrote his well-known axiom: "all great states, particularly this kingdom, can maintain themselves [internally] only by [foreign] war." Bodin held the same view. Perhaps some conspirators and ministers conceived these seditions and the subsequent Revocation in 1685 to be final campaigns in the Wars of Religion which had always had an international dimension.

The dramatic results of Malettke's research raise problems wider than his analysis of political theory. For the extravagant enterprises of disaffected provincial nobility testify more to a rejection of *la civilisation française* as understood by members of *la cour et la ville* than they manifest adherence to this or that constitutional principle. In fact, the conflict was part of a process of acculturation which historians are only beginning to perceive.

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PIERRE GOUBERT. *Clio parmi les hommes: Recueil d'articles*. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques, Civilisations et Sociétés, number 52.) Paris: Mouton, 1976. Pp. 310. 68 fr.

In France the republication of a corpus of articles is an honor generally accorded to distinguished scholars nearing retirement. Since Pierre Goubert is one of the most influential social historians since Marc Bloch and Lucien Febvre (and one of the best known French historians in the United States) this volume is richly deserved. Its thirty selections, a spectrum of essays, reviews, and debates dating from 1952 to 1975, have been reprinted without alteration. Although the collection as a whole is sometimes repetitious, its individual essays (many of which were previously published in hard-to-

locate symposia) maintain a very lively interest. They touch on all the principal concerns of this earthy, earnest, ambitious yet rigorous researcher of early-modern France: peasants and their livelihoods, rural society, prices, merchant fortunes, demography, institutions, social structure. Admirers of his books risk a certain feeling of *déjà vu*, for as the author points out, these occasional pieces were never intended as more than progress reports. But since for Goubert the *process* of discovery is almost as important as the findings, the articles provide a fine exposure to his seemingly effortless method of moving from series of intensively examined documents to the reconstruction of the lives they reflect and then to the regional patterns these suggest.

We can follow Goubert from the much debated discovery of how bad things were in the Beauvaisis to his realization years later that in Brittany things were completely different. Later, in "*Sociétés rurales françaises du 18^e siècle*" (1974) he brilliantly synthesizes the multitude of regional variations which had since come to light. A 1969 essay offers less familiar ponderings on the seventeenth-century "crisis," fiscal wealth, and Parisian construction, while two telling essays punch holes into Roland Mousnier's "society of orders." (1969, 1973) A number of essays treat aspects of demography from the sixteenth century forward, and an unpublished piece from 1973 analyzes the question of vagabondage in the Paris region at the end of the eighteenth century. Through all this and much more, Goubert returns repeatedly to the same fruitful, at times exasperating, themes: that the study of the most humble and numerous groups is essential; that the unit of analysis must be the human community viewed locally and grouped regionally; that all abstractions are dangerous; that reality lies in patiently constructed *series* of *quantifiable* documents; that the *Annales* historians have produced "everything worth reading in French historical work for almost half a century" (p. 7).

The book is especially interesting as a source for the history of what Goubert mischievously calls the "groups of the *Annales*." Through the essays we can watch the rise and fall of concepts like the "sad seventeenth century" and clock the progress of luminaries like Le Roy Ladurie, Deyon, and Lebrun, rising from "young scholars working in the field" to become authors of newly devoured *thèses* and then reigning masters. This retrospective consciousness, which is enhanced by the 1974 postscripts on some of the articles, is developed in the stunning "*Sur trois siècles et trois décennies: passage des méthodologies*" (1972) which races through the past thirty years of fervent thesis-making in an almost apocalyptic recapitulation of hypotheses raised,

ideas modified, and new questions posed. The collection as a whole is fascinating. No single monograph could give such a good historical perspective on what this sort of history can be, and what it is not.

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LENARD R. BERLANSTEIN. *The Barristers of Toulouse in the Eighteenth Century (1740-1793)*. (Johns Hopkins University Studies in Historical and Political Science. Ninety-third Series, number 1.) Baltimore: Johns Hopkins University Press. 1975. Pp. xii, 210. \$12.50.

The professional function of the barristers (*avocats*), their social status, and their chances for social mobility occupy the first half of this work, the essential traits being established by statistical analysis and illustrated by anecdotal examples. The features of the collective biography of a socioprofessional order that emerge are made sharp by comparisons with its next inferior group, the attorneys (*procureurs*), its equal, the bourgeois (*rentiers*), and its near superiors, the noble *parlementaires*. Just seven percent of the more than two hundred registered barristers of Toulouse in this period did most of the pleading in the Parlement, the rest lived chiefly off income from family fortunes (about two-thirds in land, one third in *rentes constituées*), and some did not practice at all. For the barristers who were active, their social status as commoners (with a few exceptions) seems to have concerned them less (probably because they were the élite of the third estate) than their professional association with the noble magistrates of the sovereign court. They rarely rose to noble status by the devices of marriage, enormous wealth, or pursuing venal/hereditary offices, but sometimes they made it by appointment to the ennobling Toulousan office of the *capitoul*.

The second half of the work places the model barrister in the intellectual life of the times (mores, libraries, academies, legal reform movements, social thought), studies his function in Toulousan society and politics (Penitents and Freemasons, municipal reform movements, reaction to Maupeou), and finally scrutinizes his conduct during the first four years of the Revolution. In the last of these, the author tests long-established premises about the role of lawyers in the Revolution.

In Toulouse, the aristocratic reaction of the late 1780s was led by *parlementaires* in opposition to the Estates of Languedoc, where more than half the representatives were commoners. Most barristers supported their parliamentary colleagues rather than their social equals, but their dilemmas were many. One of them, a noble whose son sat in the

second estate, chose to sit in the third estate and campaigned for its doubling. He voted to abolish most noble privileges, but not to undermine the noble estate as such, and he turned against the Revolution by the end of 1789. Even the half dozen or so truly revolutionary barristers of Toulouse who served the government during the 1790s (some fifty-four of them did so in some capacity at some time) were lax in enforcing revolutionary programs which broke sharply with tradition. In fact, the profiles of age, wealth, and status of those who supported the Revolution provide no basis to differentiate them from their colleagues who opposed it. Most of them disdained pleading in the new courts, which were stripped of aristocratic trappings; many of them took up their careers again during the Napoleonic period. The group as a whole seems to have dwelt on the balance point between the old regime and the new, as few groups did. The lawyers of Toulouse who served the Revolution best came from the Seneschal Court, the Parlement's cross-town "inferior" court rival.

The things just said do scant justice to the wealth of information this work offers to students of the social history of the *ancien régime* and the early Revolution, be they of a humanistic or a social-scientific bent of mind. This excellently researched and analytically well-framed study must appeal to them all.

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JACK RICHARD CENSER. *Prelude to Power: The Parisian Radical Press, 1789-1791*. Baltimore: Johns Hopkins University Press. 1976. Pp. xiv, 186. \$12.50.

Jack Censer's monograph provides a solid reference point for discussion of radicalism in the French Revolution—what might be called "revolution in the Revolution." While he situates his work in a number of historiographical contexts, his most obvious purpose is to provide one missing link in the chain of politicization described so vividly by George Rudé's *The Crowd in the French Revolution* (1959). For all Rudé's stress on the subsistence issue in mobilizing revolutionary crowds, his work made clear that a growing political consciousness was equally important. Yet Rudé said little about its sources, content, or dissemination.

Censer convincingly hypostasizes one such source—a group of "Cordelier journalists"—all of whom shared the militant attitudes of the Cordeliers district and club, and most of whom lived or worked in that neighborhood or belonged to the club. Specifically he analyzes six small-format radical newspapers that appeared during the period of the Constituent Assembly and that were long on opinion while relatively short on news. It is

very fruitful to group these newspapers together and to recognize that Marat's *Ami du Peuple* and Prudhomme's enormously successful *Révolutions de Paris* belonged to what was virtually a self-conscious fraternity of radical journalists. Of course, this procedure necessitates considerable methodological discussion and leaves the book weighed down with the ponderous detail of content analysis. But the job is well done and is necessary to validate Censer's method of extracting the pure ore in these newspapers—namely, the journalists' preoccupation with "the ideology of popular sovereignty."

True, his discussion of the journalists' backgrounds and of possible explanations for their radical stance is not especially illuminating, in part because it is based on rather skimpy research. Moreover (and this is not a criticism of Censer, but rather a warning the reader deserves), the book sheds almost no direct light on how and how far the influence of these newspapers spread. His subject is their content, and he exploits it very well. While one might quarrel with his choice of the term "ideology" in favor of a looser and less complex term, Censer leaves no question about its substance. Indeed, his chapter on that subject should be required reading for anyone interested in the French Revolution. The analysis may not sound unfamiliar, but it is done systematically and thoughtfully, and covers a period earlier than is customary for consideration of concepts of social equality and popular sovereignty.

The radicals' basic image was of a society polarized between a large, well-meaning, moral, and freedom-loving "People" and a small, self-centered, egotistical "Aristocracy" seeking to monopolize wealth and power in the new regime. The radicals were concerned with political and moral issues; they had little to say about property or questions of political economy. Their concept of the "People" was purposefully vague, while that of the "Aristocracy" was provocatively inclusive—attacking *les grands* (be they royal, noble, or bourgeois), the luxuriously rich, the non-parish clergy, and (interestingly) the lawyers: all those who allegedly used their power to control the lives of others or to acquire for themselves a disproportionate share of the benefits conferred by the social order. At first Censer's summary of motifs, arguments, and quotations from the radical press makes this ideology sound alternately banal, mythic, even paranoid, and perhaps to a certain extent it was all of these. But gradually, as this simplistic image is fleshed out, it becomes increasingly comprehensible in terms both practical and idealistic. For all its vagueness and exaggeration, was this sense of a "People" versus an "Aristocracy" so far removed from Pierre Goubert's

analysis of the cleavage in Old Regime society between *dominants* and *dominées*?

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JACK BAKUNIN. *Pierre Leroux and the Birth of Democratic Socialism, 1797–1848*. New York: Revisionist Press. 1976.

Pierre Leroux was an early Utopian socialist philosopher. His ideas may have impressed his contemporaries, but they seem to have had little lasting consequence and little resemblance to socialist theory since judged to be consequential. Hence, historians have not usually paid much attention to the man, though most specialists could probably identify him. Jack Bakunin's careful and intelligent analysis of Leroux's thought makes him appear worthy of attention and respect.

As a young man during the Restoration, Leroux was a passionately idealistic liberal. Rapidly disillusioned by the July Monarchy, he devoted himself to the Saint-Simonian movement for a year, then left it to evolve his own distinctive socialist philosophy. Believing that ideas rule history, he avoided nonintellectual political activity, except for serving as a deputy in the Second Republic. He played no large role then and lived in obscurity afterward, dying in Paris during the rule of the 1871 Commune.

Leroux developed a religion of humanity, as a new faith needed to replace outmoded Christianity and to give man a new moral vision. This would make possible transition to a socialism based on full equality and individual liberty, in social and economic conditions as well as political rights. Capitalism would be abolished, and workers would control the places where they worked. Leroux based all this on a philosophy of history seen as the progressive movement of ideas. Economic and social development were only the result, not the catalyst, of this movement.

In retrospect his theory can seem hopelessly Utopian, in the word's pejorative sense. Yet Leroux lived in a time when successive attacks on earlier beliefs had left too little foundation for ready construction of solutions to felt human problems. Many answers that appealed to contemporaries violated his principles, for example, Saint-Simonian hierarchical social schemes. He had to grope where few had preceded him, yet he managed to conceive much of what would later become the basis of modern socialism. Bakunin's book lets us appreciate what a creative effort his was and, more generally, the difficulty of developing innovative ideas in such an era.

Bakunin presents very clearly the thought both of Pierre Leroux and of others immediately rele-

vant to the study of his thought. The form of his exposition makes me fear that the author occasionally misleadingly blends his own thinking with Leroux's, but otherwise Bakunin has achieved his purposes quite ably. He has defined his subject too narrowly, however, for the book to come fully to life. We are shown ideas without seeing much of the men who conceived them or the context in which they arose. This might have been a still better book had it looked further, but it is solid enough as it is.

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FERNAND BRAUDEL and ERNEST LABROUSSE, editors. *Histoire économique et sociale de la France*. Volume 3, *L'avènement de l'ère industrielle (1789–années 1880)*. In two parts. Paris: Presses Universitaires de France. 1976. Pp. 471, xxxv; 475–1071.

The *Histoire économique et sociale de la France* published under the direction of Fernand Braudel and Ernest Labrousse begins with the Middle Ages and continues to the present day. The entire undertaking is as ambitious as it is necessary. No overview of French social and economic history has been written since the foundation of the *Annales* school, with the result that much recent work has been available only in monograph form. A synthesis has been needed to link these studies together, and to draw general conclusions from them.

The volume in question, which covers the period between the French Revolution and the mid-1880s, fulfills all expectations. Nine eminent French historians have collaborated in its preparation, each approaching the topic of the mutations in French economy and society from a different angle. The unifying theme of the study is the question of growth, and the influence of the work of the *Institut des Sciences économiques appliquées* is evident throughout the thousand pages of this book.

The construction of the railroad system was at the base of nineteenth-century economic growth. Pierre Léon analyzes the extension of transit systems which led to the creation of a truly national market. Before the advent of adequate transportation, farmers feared a too abundant harvest which would only lower prices; "*l'année rêvée [était] l'année moyenne*" (p. 621). But once farmers could count on selling their surpluses outside the immediate area, production decisions could be based on national needs. With the possibility of transporting foodstuffs by rail from one part of the country to another, the intensity of old-style demographic crises was muted. As André Armengaud notes in his section on demography, bread prices were no

longer an essential factor in determining patterns of mortality and natality. After 1860, the only fluctuation in the price of wheat capable of influencing the number of births was one of over twenty percent.

French industry grew much more rapidly than French agriculture in the 1789–1880 period, and Pierre Léon contributes four important chapters analyzing and measuring this growth. This section relies heavily on series of data on production collected by T. J. Markovitch, J. Marczewski, François Crouzet, and Maurice Lévy-Leboyer. Industrial growth is seen as occurring most rapidly in the 1835–44 and 1855–64 decades. The idea of an economic boom under Napoleon III will be familiar to most readers; the notion that “*La prospérité, c’est . . . Louis-Philippe, malgré les malheurs de la fin*” (p. 983) is a newer one.

Robert Laurent contributes an excellent chapter describing how these many economic changes influenced the life of the peasantry. Agricultural salaries rose considerably after 1851, largely because of the rural exodus, and this, along with the end of grain crises, led to a “*dédramatisation relativement rapide de l’histoire rurale*” (p. 766). Adeline Daumard supplies a cogent essay on the social psychology of the bourgeoisie and middle classes, and traces the changes in their composition throughout the period.

The usefulness of this outstanding book is enhanced by an annotated bibliography and many handsome graphs and maps. In the best French tradition of collective scholarly effort, this study is indispensable reading for the historian of modern France.

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SEYMOUR H. MAUSKOPF. *Crystals and Compounds: Molecular Structure and Composition in Nineteenth-Century French Science*. (Transactions of the American Philosophical Society. New Series, volume 66, part 3.) Philadelphia: the Society. 1976. Pp. 82. \$4.50.

The mechanical philosophy of the seventeenth century was based upon the idea that the phenomenal world was nothing but the result of the mechanical actions of the ultimate particles of bodies. Since these ultimate particles were not sensible, there was a philosophical problem here of the utmost importance. Sir Isaac Newton faced this problem squarely in the third of his “Rules of Reasoning” by suggesting that we can know the properties of these particles by discovering the universal properties of gross matter and assuming that these properties *must* be those of atoms as well. This principle of transduction, as it has been chris-

tened by modern philosophers, was seductive but it just didn’t work. By the end of the eighteenth century there were those who felt that it was hindering the further advance of science and who therefore dispensed with it. Lavoisier noted in the preface to his *Elementary Treatise on Chemistry*, which announced the chemical revolution, that as far as the properties of the ultimate particles of bodies were concerned it was probable that we know nothing whatsoever about them. And, he strongly implied, the time had come to give up metaphysical models that could only serve to distract chemists from the real chemical problems of analysis and classification. That, in a very real sense, was what the chemical revolution was all about.

Unfortunately for Lavoisier’s revolution, but fortunately for chemistry, few chemists could avoid thinking about atoms, their sizes, shapes, and forces. But with no valid principle whereby atomic properties could be determined in the laboratory, such thoughts had to remain fuzzy and imprecise. Yet in less than a century after Lavoisier’s treatise was published (1789), J. H. van’t Hoff, Auguste Kekulé, and other chemists could confidently speak of the correlation between chemical properties and molecular form. The empirical way to knowledge of atomic and molecular properties had been built in three scientific generations. The early gropings of crystallographers and chemists in France from the last decades of the eighteenth century to Louis Pasteur’s epoch-making discovery of enantiomorphism (left-handed and right-handed asymmetry in the crystals of tartaric acid) form the subject of Seymour H. Mauskopf’s exemplary study. What he does is to trace, step by step—and the steps are technical, demanding close attention and requiring some knowledge of both chemistry and crystallography—the evolution of ideas in France about elementary molecular structure from the pioneering work of Haüy and Romé de l’Isle, through Ampère, Beudant, Biot, Dumas, and others to Pasteur.

The ideas that are traced are overwhelmingly drawn from published sources; only in the case of Pasteur is there any serious use of manuscript remains to help elucidate the main theme. This is not meant as a criticism, for Mauskopf quite rightly shows that the published ideas were, in fact, the important ones in the development of ideas of molecular structure. But reliance on published sources does obscure one noteworthy theme. The problem of transduction was a serious philosophical issue in nineteenth-century France, and what lies behind the apparently austere scientific writings of Mauskopf’s actors is an exciting debate over the philosophical foundations of molecular physics and chemistry. The debate, unfortunately, tended to rage within the breasts of those wrestling

with the problem. Ampère, for example, struggled for years to devise a philosophy that would permit him to create the kind of "metaphysical" atomic and molecular models he felt were necessary for the kind of science he wanted to do. This struggle can be found only in his unpublished papers. I suspect that this is true of the others involved as well.

Within the limits that Mauskopf has set himself, he has written an exciting and stimulating account. My criticism can be aimed only at very minor points. Too many misprints indicate carelessness in proofreading. Some of the citations are also liable to confuse those not working directly in the same area. Why does Mauskopf abbreviate the *Annales de chimie* as *Annal. de chimie*? He saves only one space and creates possible confusion. More important, he does not indicate to the reader that there was a change of series in this publication. Thus the reader may wonder how volume 14 (1820) can possibly follow volume 90 (1814). The index is imperfect and, again, shows signs of haste and carelessness. Still, this is an important monograph that is absolutely essential for anyone working in the history of nineteenth-century molecular physics.

L. PEARCE WILLIAMS
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JAMES M. LAUX. *In First Gear: The French Automobile Industry to 1914*. Montreal: McGill-Queen's University Press. 1976. Pp. x, 239. \$22.50.

James M. Laux has assembled a factual chronicle of the formation and evolution of some forty-five significant Parisian and provincial auto firms from the 1890s to the First World War. This wealth of detail concerning sales, profits, financial reorganizations and the style, size, and power of models is available in no other single volume. *In First Gear* will delight antique car buffs and prove a valuable reference work for scholars interested in the industry's relationship to French economic development or the social role of the auto during the *belle époque*.

Unfortunately, Laux belies his preface by failing to probe such areas here. He concludes that before 1914, "a potentially large mass market for cars did exist in France" (p. 201). Businessmen could have tapped this market by rationalizing production and slashing prices. Such a hardy claim requires the firm support of evidence gathered from economic and social history. This book lacks such evidence. Given the wage structure of prewar society and the absence of installment purchase facilities, it is hard to see who among workers or the petty bourgeoisie could have afforded autos. Cars had not yet become real or imagined necessities:

even economy models offered by several manufacturers enjoyed no smash success, as Laux makes clear. French automakers, after a brilliant start, refused to emulate Henry Ford. Why?

Laux sheds little light upon this, the critical question. Auto production in France remained labor-intensive. What explains this best? Is it: 1.) The high cost of metallurgical raw materials and thus expensive French machinery (there is no reference to the tariff preferences of the *Comité des Forges*); or 2.) industrialists' reluctance to seek outside financing (relations between banks and automakers are not explored systematically; we are merely told [pp. 84, 205] that the industry "was not starved of capital"); or 3.) sheer lack of sustained entrepreneurial vigor? Some automakers were dynamic (pp. 67, 90), but others were not (pp. 117, 159). Indeed, with the exception of Citroën pleading with stockholders, Laux shows us no other automaker actually at work, whether in the shop, the boardroom, or negotiating with labor. Management strategies are not explored. Relationships among automakers and between them and other businessmen are either ignored or treated at the anecdotal level.

The early auto was the plaything of the *sportif* and the very rich. Some (how many? why?) among the professional classes began to perceive the utility of the car. Perhaps French management viewed these groups as the most proper market for their product. If so, the new industry functioned in France as a means of reinforcing social stratification, not eroding it as in the United States.

The distinctive development of the auto industry poses a whole range of questions about the structure and vitality of the French economy, the socioeconomic attitudes of innovative and traditionalist businessmen, and, at least by implication, the very nature of a mature capitalist society. Laux offers useful data for the historian who will address such problems.

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JEAN NORTON CRU. *War Books: A Study in Historical Criticism*. Edited by STANLEY J. PINCETL, JR. and ERNEST MARCHAND. San Diego: San Diego State University Press. 1976. Pp. xi, 194. \$5.00.

In 1929 Jean Norton Cru published "*Témoins*," a monumental study of nearly three hundred texts—diaries, memoirs, reflections, novels—written by French soldiers about their experiences during the First World War. Cru was not concerned with the literary quality of the work he was examining but with its value as a historical source. In order to

establish this he carefully checked the service records of his witnesses so that their claims to have participated in any given action could be verified. At the end of his book he published a series of lists classifying his authors according to the accuracy of their testimony. Cru's conclusion spared neither the Left nor the Right: although his own hatred of war was made abundantly clear in the book he was as critical of the exaggerations of antimilitarists like Barbusse as he was of those of the Nationalists.

Cru's object was to destroy all the myths and the rhetoric associated with war, and he was anxious that this message should be broadcast as widely as possible. Since "*Témoins*" was over seven hundred pages long he published an abbreviated version entitled "*Du Témoignage*" in 1931. It is Cru's own translation of this second book that has now been published by the San Diego State University Press. Inevitably a great deal has been lost in the process of abridgement. Shorn of many of the quotations with which he buttressed his arguments, many of Cru's judgments now appear somewhat summary and disjointed. And it is a great pity that the editors have not provided a longer introduction which could have helped English-speaking readers to appreciate the terrible physical and psychological wounds that France suffered during the First World War.

Still, the publication of this book has been an eminently worthwhile undertaking, and the editors are to be congratulated for including a brief but fascinating biography of Cru by his sister. From this account Cru emerges as a passionate individualist, the most important period of his life being the four years that he spent during his childhood living with the natives of the island of Maré in the Loyalty group, not far from New Caledonia, where his father was a Protestant missionary. In regard to his book Cru once wrote: "'Witnesses' is indebted to the little primitive of Maré who lived on in me and who survived the attempts of conformity, in the school, in the regiment . . . at the front." He and his book deserve to be more widely known.

FRANK FIELD
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ANTHONY TIHAMER KOMJATHY. *The Crises of France's East Central European Diplomacy, 1933-1938*. (East European Monographs, number 21.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York, 1976. Pp. vi. 277. \$14.50.

This book is neither a monograph based on archival research nor an interpretative essay characterized by fresh insights and thought-provoking for-

mulations. With one exception Anthony Tihamer Komjathy relies entirely on published sources—rather incomplete at that—and provides the reader with a study of what he regards as the six major crises in East Central Europe from Hitler's accession to power to Munich.

Two background chapters outline the situation of East Central Europe and of France; several deal with the crises; two with Barthou's and Laval's foreign policies; one is called "The End of Collective Security" and the last but one "The Defeat at Munich." "Final Observations" serve as a conclusion. This organization while essentially chronological is also partly geographical, and the author tends to move back and forth, a method which can be confusing. A subdivision of chapters into short sections does not add to clarity of exposition. The content of the book is disappointing, for Komjathy's treatment of the subject is superficial and his analysis uninspiring. The author's technique of spelling out various foreign policy alternatives, offering advice, and distributing blame and praise is rather unsophisticated.

The bibliography is inadequate. Basic documentary collections—even some cited in footnotes—are missing, and there are gaps in memoir and secondary literature. While monographs are arranged by country there is no section on France, which is astonishing. Editorial work has been poor and errors, slips, and misprints abound. This, in addition to many dubious statements and incorrect assertions, often makes the reading irritating.

The concluding chapter contains some speculation and general theses. Unfortunately these are either somewhat obvious or dubious. The assertion that economic considerations made East Central European states depart from a pro-French line and seek political rapprochement with Germany is simplistic, and Komjathy weakens his argument by ignoring French investments in and loans to East Central Europe as a factor in Paris' foreign policy.

It seems clear that the author took on a far bigger task than he was prepared to handle. The sloppy editorial work made matters worse, and one wonders if the publication of this monograph in its present form was not really a disservice to the author.

PIOTR S. WANDYCZ
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JOHN F. SWEETS. *The Politics of Resistance in France, 1940-1944: A History of the Mouvements Unis de la Résistance*. DeKalb: Northern Illinois University Press, 1976. pp. xii. 260. \$12.50.

The number of books on the French Resistance during World War II is rapidly growing, but there

are still relatively few good scholarly studies in English. John Sweets' book is a worthwhile addition to the literature even though it limits itself to only one part of the French Resistance movement. That part is the MUR (*Mouvements Unis de la Résistance*), an amalgam of the three leading southern underground groups (*Combat*, *Libération*, *Franc-Tireur*), which combined their networks in 1942, and whose adherents hoped to provide, by their success, a model for total unification.

The ultimate failure of total unification and the growing complexity of political and military problems as the 1944 landings drew near render all histories of the French Resistance difficult. But Sweets labors under an additional difficulty, that of elucidating MUR relations with a variety of elements—the northern movements, the military groups, the external leadership. Because these other groups are not clarified in equal detail, the reader occasionally wishes that Sweets had chosen to provide what the title suggests, a complete overview of Resistance politics.

So far as method goes, Sweets' approach to his subject is admirable. He first provides a summary of how the three southern movements developed—*Combat* under the dynamic leadership of Henri Frenay, *Libération* molded by the left-leaning journalist Emmanuel d'Astier de la Vigerie, *Franc-Tireur* guided by the Jewish technician Jean-Pierre Lévy. Sweets describes how these clandestine groups, so diverse in organization and leadership, nevertheless found such advantages in union that they were able to merge, under Gaullist leadership, in 1942. The author then turns to the relationships which the MUR maintained with de Gaulle, with the Communists, with French political parties. These sections of the book are extremely useful because they explore and elucidate in depth the main streams of French political concerns during the war. In these sections and in the final one—on the liberation—the author strains at his self-imposed limitations, because for 1943 and 1944 he can hardly deal with the MUR alone, eclipsed as it was by larger issues. Even the MUR leaders Frenay and d'Astier had by this time joined the external Resistance, leaving the author without colorful protagonists on which to focus his commentary.

Although a broader spectrum might have made this volume more useful to the general reader, nevertheless one can find only praise for the scholarly craftsmanship of Sweets' study, which was begun as a doctoral dissertation at Duke. In the course of his research the author had access to documentation available only in Paris: materials assembled by Henri Michel's *Comité d'Histoire de la Deuxième Guerre Mondiale*. Using these archives, together with numerous interviews, Sweets has produced a meticulous study which compares favor-

ably with the best in French scholarship. His final assessment of the value of the Resistance is a measured approach using most of the information currently available. As an essay on frustrations and heartbreaks, as well as successes and glory, this last chapter can profitably be read without reference to earlier sections.

ARTHUR L. FUNK
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La libération de la France: Actes du Colloque International tenu à Paris du 28 au 31 octobre 1974. Paris: Centre National de la Recherche Scientifique. 1976. Pp. 1054. 180 fr.

Recent years have witnessed great progress in the study of France during the period of the occupation, Resistance, and Liberation. French and foreign scholars have overcome the tendency not to stir up troubled waters. Much of the credit for the development of serious scholarship must be given to Henri Michel and the *Comité d'Histoire de la Deuxième Guerre Mondiale*.

In October 1974 the Committee held a four-day colloquium on the Liberation of France, with three hundred participants. The text of the colloquium has now been published, and it testifies to the present state of understanding of 1944. The volume contains about one hundred written reports and oral presentations. The scope is enormous, the contents eclectic. Naturally, the great questions receive the most attention: the role of de Gaulle and the Communist Party, the respective situations of Vichy and the Resistance, the military problems following D-Day, the restoration of republican legality in the provinces, the political and economic problems following the Liberation. Other important topics are treated in some detail, for example, the liberation of Corsica in 1943, which was in many ways a test case for what would occur later in the metropole.

Each section is divided into a major report, a reply, and several shorter communications. This approach is unequally successful. Many of the major essays are excellent (Agulhon on the P.C. for one), but others are merely summaries of the authors' long-since published works or banal expositions of *lieux communs*. Because the volume is a complete transcript of the meeting, some papers of low quality have not been eliminated. Furthermore, a certain repetitiveness is unavoidable because participants did not always know beforehand what others would say. Yet one should not allow these faults to overshadow the immense value of the volume.

Here are, in brief and concise formulations, the results of a great deal of good research. We are struck by how much the level of the discussion has been raised in recent years. Although differences

of interpretation abound, one senses a general movement toward broad consensus. For one thing, no one now asserts that the P.C. was planning a revolutionary seizure of power in 1944. That is progress; now it is possible to examine the problem of the Communists in depth.

The fascination of this book derives from a leitmotif that runs all through it: the contrast in perspective between the historian, writing *a posteriori*, and the political actor. In the long run, there seems to be much more in common between two resisters, whatever their political position, than between a given resister and a historian. René Hostache brilliantly analyzes the structure of the Resistance in 1944. Pascal Copeau objects that these elaborate organograms do not correspond with things as he saw them. Debré dwells on the paradox: the structure of the Resistance was clear above all after the Liberation. And in a final comment, Copeau addresses the historians: "... when we find our community (*cité*) in your studies, my dear young researchers, it appears a little icy. Don't be afraid ... to steep your pens in blood, because behind each of the initials which you explicate with much bookish learning, there are our dead comrades."

In contemporary history we have the advantage of participants who can still talk back, who tell us that history cannot be written without taking into account the world subjectively known to them. It is from the confrontation of scholar and participant that great history can be written. This bulky, uneven, irritating volume provides passionate reading for those who wish to understand not only the Liberation, but the very creative process of historiography.

STEVEN PHILIP KRAMER
University of New Mexico

PATRICK J. SCHAEFFER. *L'Alsace et l'Allemagne de 1945 à 1949*. (Centre de Recherches Relations Internationales de l'Université de Metz, number 8.) Metz: Centre de Recherches Relations Internationales de l'Université de Metz. 1976. Pp. x, 388.

Within the framework of present-day interest in regional studies, this reworked "*thèse de doctorat du troisième cycle*" at the University of Metz takes a very special place, since Alsace, its history, and its relations to Germany in war and peace present some very special problems.

The study is divided into four parts, of which the first three deal with the legacies of the German occupation. In the first section indemnification of prisoners of war (often returning from Russian camps), civilian internees, and members of forced labor battalions are covered, as are property restitutions. The second section concentrates on the

reintegration of Alsace into the French community. Here the author stresses that no special allowances were made by the central government since the fiction was maintained that French legality covered the years from 1940-45. Nevertheless, legal problems of marriage and insurance contracts arose and are outlined, the purges of collaborators and war crimes trials are discussed, as is the whole complex of questions associated with the teaching of French and German in school. Consideration is given here also to the bilingual press, as well as to the presentations on radio and cinema. Paradoxically it turns out that the older population preferred general entertainment films in German, with French subtitles added for the younger members of the audience. The third section concentrates on territorial adjustments, particularly on the competition of the German harbor at Kehl with the harbor at Strasbourg, on the use of German prisoners of war in the labor force, and on minor frontier alignments.

With all this necessary background made available, only the fourth section deals at length with Alsatian perceptions of postwar Germany, first as the conquered enemy, then as the pawn between East and West in the Cold War, later as the resurgent economic power, and finally as the possible partner in a European union. Each period is characterized by deep Alsatian mistrust, which, however, is differentiated according to party, the socialists and center democratic groups being more willing to cooperate with the emerging West German state, whereas the Gaullists and the Communists were taking an uncompromising stance toward it.

The author regretfully concedes that he did not have access to departmental archives, and that his entire work had to be based on published documentation and a careful evaluation of the Alsatian press, French and bilingual. The study is presented with restraint, clarity, and an admirable lack of special pleading. Careful footnotes (at the bottom of the page) and an extensive bibliography (only Charles Louis Foulon, *Le pouvoir en province à la libération* is not mentioned; it discusses the work and influence of the "*commissaires de la république*" at the critical time of liberation) make it a ground work upon which future detailed investigations can be built once the French departmental archives on a "Thirty Years Rule" are available.

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RICARDO GARCÍA CÁRCCEL. *Orígenes de la Inquisición española: El tribunal de Valencia, 1478-1530*. Prologue by HENRY KAMEN. (Serie Universitaria: Historia, Ciencia, Sociedad, number 132.) Barcelona: Ediciones Península. 1976. Pp. 306.

Space limitations prevent the discussion which this pioneering study merits. Author of a recent valuable monograph on the *Germanía* rising, Ricardo García Cárcel sustains the traditional view that Ferdinand used the Inquisition to reduce the *fueros* of the eastern provinces. Charles V's reaffirmation of the institution by 1521 continued that process, interwoven with his more imperial considerations on the need for socio-religious homogeneity. Thus the hopes and requests for inquisitorial reform, promoted by Pope Leo X in informal concert with the *conversos* and the eastern Cortes (though they acted for dissimilar reasons), failed.

This is not to underestimate the purely religious element; Ferdinand replied to one Valencian complaint that the Inquisition overrode both provincial *fueros* and individual rights by observing that no *fuero* could justify ignoring heresy, even at cost to the economy. The Inquisition vigorously promoted anti-Semitic theological tracts, while sharing with the crown revenues from fines and confiscations. The religious and the fiscal perhaps could never be separated. Further, the astounding absenteeism of the bishops of Valencia from 1452 to 1538 assisted regalism: from 1484 Ferdinand appointed the see's vicars-general from one of the two provincial inquisitors. García Cárcel concludes that through the new Inquisition Ferdinand had an "economic-religious entity in which he was actually a feudal seigneur" (p. 45).

Over ninety-one percent of those processed were judaizers, which is testimony to the success of the Christian lords in protecting their Moriscos until 1609. An unparalleled third of Valencia's people had Semitic origins and most of these were Mohammedan. The author sustains Netanyahu's controversial thesis that late medieval Hispanic Jewry was internally corroded by secularizing intellectual currents; "Valencian *conversos* inherited from Judaism its ritualism alone," while in one tantalizing case the condemned was confusingly "firm in neither faith" (pp. 195-99). The Valencian Inquisition decapitated the *converso* bourgeoisie and artisans; in particular, a cluster of families responsible for continuing medical practice and science was virtually eliminated. What prospects for discussion of the well-known scientific regression of Spain might this offer?

All students of the Inquisition, Renaissance Spain, and the interaction of institutions and values can profit from this meticulously researched, clearly executed work, which lacks only a helpful conclusion.

PAUL J. HAUBEN
University of the Pacific

I. A. A. THOMPSON. *War and Government in Habsburg Spain, 1560-1620*. London: Athlone Press; distrib-

uted by Humanities Press, Atlantic Highlands, N.J. 1976. Pp. 374. \$20.25.

Along with Geoffrey Parker's recent *The Army of Flanders and the Spanish Road*, I. A. A. Thompson's study of military administration under Philip II and Philip III provides an up-to-date look at the legendary institution by which the Spanish Habsburgs dominated Europe. Mention of Parker's book is useful because Thompson largely excises the Netherlands war from his book as "an irrelevance" from the viewpoint of Castilian administration. The book, based on serious archival research and extensive reading of printed materials, establishes its author's judgment as deserving of respect. Yet telling this story without the northern war sometimes seems like trying to concentrate on two rings of a three-ring circus—it complicates things rather than simplifying them. Thompson concerns himself with both naval and army affairs, Italy and Africa, and specifically with financial and recruiting problems where demands of the northern war were sometimes of paramount importance. Another dimension largely missing from the book is consideration of adjustments to the changing nature of land warfare, again something to which Parker gives considerable attention. Thompson's focus is on the bureaucracy, but he does incorporate changing techniques of naval warfare insofar as they concerned administrators.

Both as a synthesis and in its detail, the book has much that is new and important. Thompson establishes certain points beyond cavil: the importance of military administration as a key to understanding the whole functioning and direction of government; the shaping of administrative policy more by the demands of war than by official policy. He also successfully blurs the lines that separate the reigns of Philip II and his two immediate successors, finding patterns in the continuity and change of administrative problems and policies rather than in changes of kings and their advisers. More specifically, royal efforts toward central control, *administración*, yielded to the need to resort to local control and contracting, *asiento*, which in turn helped to undermine broader efforts toward government centralization. Nonetheless, a familiar picture does survive. Philip II pushed a heroic, if unrealistic, concept of administrative centralization to serve his heroic and unrealistic policies. He and his successors increasingly had to adjust concepts to fit actual supplies of men and money. At least for the sake of argument, the process of shifting back and forth from *administración* to *asiento* might serve as a sign of healthy adaptation, however reluctant, rather than failure. In this adaptation one might find a clue to the survival of monarchy and empire.

None of the above should be understood as a denigration of Thompson's book, which is a careful and solid investigation of a complicated problem that has long needed investigation. Interested scholars will long be grateful to him.

PAUL STEWART
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MICHAEL R. WEISSER. *The Peasants of the Montes: The Roots of Rural Rebellion in Spain*. Chicago: University of Chicago Press. 1977. Pp. xi, 143. \$11.50.

Interest in the history of rural society and agriculture in Spain during the Old Regime has led to the publication within the past several years of a number of excellent studies. Gonzalo Anes on the agrarian history of the eighteenth century, García Lombardero on eighteenth-century Galicia, and Fernández Albaladejo on Guipúzcoa have begun to explain the causes of the economic and social dislocation characteristic of the Spanish countryside both in the early modern and modern periods. Although this study of rural society in the hill towns of the Montes of Toledo in the sixteenth and early seventeenth centuries is more limited in scope, its emphasis on the village as the unit of research confirms at the local level the conclusions of current historiography on the development and crisis of the Castilian countryside in the sixteenth and early seventeenth centuries. Through the use of village records, Michael R. Weisser has attempted an analysis of the demographic, social, and economic structure of the Montes during a significant period of Castilian rural history. Perhaps because of the difficulties inherent in the sources, this historical reconstruction of society in the Montes is uneven. The chapters dealing with demographic trends and the distribution of wealth in the area are well done and will be of most interest to historians of the Old Regime. The author has ably analyzed the expansion of the population of the Montes in the sixteenth century and the area's subsequent demographic crisis. And his discussion of the inequalities inherent in the peasant society of the Montes is effective. The chapters dealing with social life and the Church, however, are less complete. The book suffers too from the lack of a comparative approach to the problems of peasant society in a country with significant regional differences, but, on balance, this monograph on a local society is a useful addition to the growing literature in the field.

The book has a second purpose, to link the injustices of an Old Regime agrarian society to the appearance of extreme and violent social tensions in the villages in question during the twentieth century. But to what extent the inequalities of rural life in the sixteenth century contributed to

later manifestations of peasant unrest raises difficult questions of historical continuity. The author has certainly shown the existence of social imbalance in the villages of the Montes, but in the absence of a study of the area in the eighteenth and nineteenth centuries similar to the one he has done for the earlier period, the connection between sixteenth-century inequalities and twentieth-century violence must remain tenuous.

WILLIAM J. CALLAHAN
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MARIA F. NUÑEZ MUÑOZ. *La iglesia y la restauración, 1875-1881*. Prologue by VICENTE PALACIO ATARD. (Servicio de Publicaciones de la Caja General de Ahorros de Santa Cruz de Tenerife, number 37.) Santa Cruz de Tenerife: Caja General de Ahorros. 1976. Pp. 366.

There has been a welcome increase in studies of an almost forgotten segment of nineteenth-century Spain: the status and role of the Catholic Church in an era of political and social modernization. Maria F. Nuñez' work is a valuable addition to the list. Using material from the Secret Vatican and Spanish State Archives, Nuñez portrays in detail Church-state negotiations to indicate the sources of tension in the early years of the reign of Alfonso XII. By focusing on the papacy and its nuncios, on the one hand, and the Spanish episcopate, on the other, she analyzes the policies and attitudes of the Church as it struggled to emerge from one of its most disastrous encounters, the Revolution of 1868 and its aftermath. Church policies are superimposed on the actions of the Cánovas government to explore the resumption of relations between the two institutions.

Nuñez emphasizes throughout that the conflicts which characterized negotiations must be understood in an international as well as national context. The "war of ideas" between Catholicism and liberalism had placed the papacy on the defensive since mid-century. The Spanish clergy mirrored this stance and, indeed, the image for them was sharpened because the attacks they suffered during the revolution followed closely upon the Syllabus of Errors and coincided with the proclamation of papal infallibility. For its part, the Restoration state had to contend with a legacy of religious freedom (and the concomitant radical support it enjoyed), a flourishing Carlist War, and pressure from Protestant states (particularly England and Germany) to retain religious toleration as the price of recognizing Alfonso.

Within this framework Nuñez discusses restoration of the Concordat, conflicts over the new constitution, negotiations to fill vacant sees, re-establishment of religious orders, Protestant

propaganda, and economic reparation for damage suffered by the Church during the revolutionary years. She concludes that even given severe tension, especially in its early days, the Restoration favored the Church more than any other government during the century. Its good will made possible for the first time a dialogue between Catholicism and the liberal Spanish state, a dialogue which Leo XIII contemporaneously opened with the world. This "ecclesiastical restoration" was a necessary development for subsequent formation of modern Catholic political and social organizations in Spain.

In general the analysis is excellent and full. The work is a judicious appraisal of the dynamic process of pressure and counterpressure between Church and state. Nuñez' conclusion, however, needs some modification. Emphasis on the reign of Alfonso XII as the starting point for dialogue between the Church and the liberal state neglects the precedents created during the *moderado* era, 1845-68. Actions in these years provided a pattern and seed-bed for the Restoration. Stabilization of the new liberal state through Church acceptance of its existence and structure was as much a concern in the wake of the First Carlist War and accompanying revolution as in the Second. In this desire Narváez and Cánovas paralleled each other and, to a degree, the latter followed the path cut by the former. Vicente Palacio Atard, in his prologue to the book, refers to this precedent in passing; Nuñez' analysis and conclusions do not.

These comments should not, however, detract from the overall merits of the book. It substantially furthers our understanding of a fundamental mechanism of Spanish society—the relationship between the modern state and the Catholic Church.

NANCY A. ROSENBLATT
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ANGEL VIÑAS MARTIN. *El oro español en la guerra civil*. (Ministerio de hacienda, Instituto de estudios fiscales.) Madrid: the Institute. 1976. Pp. 618. 250 ptas.

The experiment in constitutional government in Spain since Franco's death has received a setback. Angel Viñas' book detailing the fate of the Republic's gold was confiscated by the Ministry of Finance a month after its publication. The government apparently moved because Viñas' interpretation slightly favors the defunct Spanish Republic. This book, therefore, is destined to end up in rare book rooms.

Viñas' previous work, *La Alemania Nazi y el 18 de julio* (1974), rightly met with a very favorable response. Viñas based it on exact citations from the

German archives, which may be checked by any international scholar. In my opinion, his *Alemania* is the best book on the Spanish Civil War published in Spain.

By Spanish standards Viñas' second book is also excellent, citing for the first time vast quantities of documents from the archives of the Banco de España and the Ministry of Finance. He includes eighty-seven charts, nine complete documents, 333 footnotes, and an excellent bibliography.

Yet if one compares the methodology of *Alemania* with *El oro*, Viñas' second book leaves much to be desired. He falls back on the typical tradition of Spanish historiography which requires the reader to trust on faith the author's interpretation and his quotations of many uncited documents. Since the ideological and class passions which partly caused the Spanish Civil War are still strong, readers should maintain maximum skepticism toward all assertions about the war. Many more notes, especially cross-references to other portions of the complex text, are needed. Moreover, certain tables could have been shortened to underline the distinction between the relevant and the irrelevant.

The substantive problem this book addresses is: how much money did the Spanish Republic spend abroad during the Civil War? Heretofore, standard works in English (by Hugh Thomas, Gabriel Jackson, Raymond Carr, Stanley Payne, Dante Puzzo, and others) have continued a false impression that Minister of Finance Juan Negrín "decided to send Spain's gold reserves" to Moscow in September and October 1936. Viñas documents the more complex reality. Since previous authors cite different amounts, Viñas' book is absolutely essential for all future research about the international aspects of the Civil War.

First Viñas concludes that the Republic spent gold in France for arms from July to October 1936 without too much difficulty before being forced by the Non-Intervention Committee and Franco's agents in France to rely mostly on the Soviet Union. Between July 1936 and March 29, 1937 the Republicans transferred some 26.5 percent of their gold to France (p. 239). This amounted to 595 million gold pesetas worth 3,894,203,386.68 paper francs (p. 66). But Viñas estimates they probably sold an additional three million gold pesetas (p. 68).

Second, the shipment of gold to the USSR in October and November 1936 consisted of 510,079,529.30 grams (Moscow's statistics, p. 201) or 510,279,293.83 grams (Banco de España's figures, p. 203), which was 72.64 percent of Spain's total prewar reserves (p. 207). This was equivalent to 1,586,236,800 gold pesetas (p. 206) or about \$518,000,000 in gold (p. 210, n. 66). The Banco de España's archives do not clarify the still con-

troversial political point of whether the Republic made its *decision* to send this amount of gold to Moscow in September or in October.

Third, most of this gold returned eventually to France. Some \$131.5 million was spent in Moscow for weapons, while Moscow transferred \$340 million to Paris to aid the Loyalists and the International Brigades (pp. 236–37; 242, n. 83; 244, n. 84; and 249).

Fourth, the state capitalists in the Soviet Union apparently retained the remaining \$46.2 million in charges for shipment, storage, insurance, bank services, and melting. The Soviets claim they advanced the Spanish Republic \$85 million worth of arms on credit in the fall of 1938, after spending the Spanish reserves.

Viñas' attempt to translate these figures into qualitative descriptions of Soviet military supplies and equipment (pp. 259–87) is unsuccessful. He produces a jumble of figures that are detailed but, except for the year 1937, almost meaningless. This section should have been omitted altogether or greatly expanded with additional research.

El oro also gives the amount the Loyalists spent in France from July 1938 to January 1939 (104,505,000 pesetas in silver, pp. 336–37) and in the U.S. from May to October 1938 (245,000,000 silver pesetas, pp. 332–33).

In summary, Viñas concludes that the Loyalists sent official reserves worth 2,200,000,000 gold pesetas (\$725 million in gold) to Paris and Moscow. Add to the gold reserves of \$725 million, \$20 million in gold for the silver coins transported to the U.S. and France (p. 346), \$3 million in precious metals and jewels collected from private sources (p. 447), and possibly another \$7 million generated by exports (p. 448, very shaky statistics) for a total of \$755 million (p. 449).

The Republic did not use all of Spain's reserves. At the beginning of the war the rebels seized 123.6 million silver pesetas (p. 33) and 7 million pesetas in gold (p. 446) in the branch banks. The Nationalist army obtained 46 percent of the Bank of Spain's silver reserve. Furthermore, the military junta of Franco captured 348,963 gold pesetas (p. 174) and 56,180,000 pesetas in silver (p. 342) from the Loyalists at the end of the war.

The last chapter, entitled "The Delivery of Precious Metals in Nationalist Spain: The Patriotic Campaign" is off the major topic of the book and is based upon the archives of the Ministry of Finance in Burgos. The Nationalists, in a campaign to collect various objects of value from private hands, managed to raise 191,323,433.24 paper pesetas by March 31, 1939 (p. 431). Viñas in this chapter touches upon the problem of how Franco financed his war. I addressed the same problem in an article in the *Journal of Contemporary History* (Jan-

uary 1977). Our accounts, based on different sources, are so completely different that the need for improved access to Spanish archives is once again underlined. As of today, both stories are mere pioneer steps—hardly definitive.

From the point of view of a general reader, one of Viñas' major weaknesses is to quote from lengthy undigested official banking statements; perhaps a third of the book consists of the driest quotations. But these quotations turn out to be a bonus for the researcher. Nevertheless, Viñas' approach makes it difficult for the reader to pick out the major conclusions from the esoteric. I wish Viñas had provided more interpretation in the text and more precise guides to the archives in his notes. Viñas assumed his readers would read this book from cover to cover in a few sittings, yet no one will. Although the book will remain a giant as a reference work, it needs many more cross-reference notes.

ROBERT H. WHEALEY
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A. J. VEENENDAAL, JR., editor. *De briefwisseling van Anthonie Heinsius, 1702–1720* [The Correspondence of Anthonie Heinsius, 1702–1720]. Volume 1, *19 maart–31 december 1702*. (Rijks Geschiedkundige Publicatiën, Large Series, number 158.) 's-Gravenhage: Martinus Nijhoff, 1976. Pp. xxxiv, 666.

This volume begins the publication of the correspondence of Heinsius, the Grandpensionary, from the death of Stadtholder William III. Heinsius tried to maintain the ruler's Grand Alliance against France. The volume under review clearly illustrates the lack of direction existing throughout the War of Spanish Succession and now aggravated by the death of the king-stadtholder. The correspondence also illustrates that the Grand Alliance was in practice rather often a euphemism for an apparently Dutch system of payments to princely military entrepreneurs.

Heinsius was at the center of a wide network of diplomatic agents and the letters received give information from Muscovy, Vienna, and other capitals. Much of the material is in French. Several of the letters highlight the presumptions of the newly created King in Prussia and his designs toward the Republic and the Nassau-Orange inheritance, while most concern military campaigns. The editor has set high scholarly standards with this first volume; reproduced are all unpublished letters, mostly *in extenso*, while all previously printed letters are inserted with their bibliographical data in the appropriate chronological place. A number of corrections on these letters are given. It is a pity that the print is tiringly small.

A. J. Veenendaal has produced an informative

biographic essay on Heinsius which should be useful to students for whom Dutch constitutional functions must often appear bewildering. In his examination of Heinsius' reluctance to serve in what in fact was a national office, Veenendaal appears to dismiss the possibility that this reluctance was a protective device aimed at providing a basis for future justifications if they were needed. Nor, in the light of subsequent developments, should it be taken lightly that Heinsius wanted to strengthen his position by being opportuned so repeatedly.

DERK VISSER
Ursinus College

ARTHUR ERWIN IMHOF. *Aspekte der Bevölkerungsentwicklung in den nordischen Ländern, 1720–1750*. In two volumes. Bern: Francke Verlag. 1976. Pp. 590; 591–1222. 180 FR the set.

This massive work deals with demographic and related conditions of the five Scandinavian countries before the improvements in agricultural techniques and the adoption of new crops, before the beginning of industrialization, and before the introduction of vaccination and modern hygiene. These countries were underdeveloped compared with contemporary England and the Netherlands except in one respect, the collection of statistical data of exceptional quality and quantity. Since the data base is most comprehensive for Sweden-Finland, Arthur Erwin Imhof deals predominantly with this area and only secondarily with Denmark, Norway, and Iceland. Although he has done archival research in all five countries, he has involved himself only occasionally in parish data and not at all in family reconstitution. Consequently he provides little information on such matters as birth intervals and none on the mean age at marriage and questions of family structure. Imhof's aim in this study is the macrohistorical investigation of those factors of demographic development that can rarely be found on the parish level.

The fundamental conditions of fluctuations in vital statistics are found to be long-term climatic and short-term meteorological changes—in Iceland also the downpour of volcanic ash destroying pasture land—as well as the incidence of epidemic and even more destructive endemic diseases, especially dysentery, pulmonary tuberculosis, and malaria. Poor harvests influenced birth, death, and marriage rates only slightly and very unevenly in different localities. Nutritional deficits were not preconditions of epidemics leading to higher levels of mortality, as E. F. Heckscher and others assumed. Between 1720 and 1750 these populations did not go through a Malthusian cycle but nevertheless experienced strong fluctuations. Until 1736

mortality was low while agricultural productivity and wages were high; yet the birth rate also was low owing to year cohorts of young parents, who had been decimated during their childhood in the 1690s. Between 1737 and 1743 mortality increased spectacularly, in three distinguishable waves from south to north. After 1743 death rates declined but not to the low level of 1720–35.

In a brief review it is impossible to mention more than a few categories of data presented in this book: vital statistics by year, by month, and according to age; changes in the disease panorama, quarantine and other health policies, epizootics, knowledge of vegetable contraceptives, migrations, and shortages of laborers. Some strikingly unbalanced sex ratios and discrepancies in periodic statistics of age groups seem to indicate unrecognized flaws in these statistics, but do not seriously affect the author's conclusions. Among peasants in southern Sweden, Imhof claims to have demonstrated the deliberate replacement of children who had died before the age of ten (*Kinderumsatz*). This is not entirely convincing, since two-thirds of these deceased children were infants, and the fact that waves of high child mortality were followed within two months by a rise in conceptions might be explained by increased fecundability after the termination of breast-feeding.

Imhof's work is rich in quantitative and descriptive data. It contains 197 tables and 110 diagrams. Yet the book is hard to use, as it is overlong, repetitious, and unfortunately lacks a subject index. Its summary is sketchy and does not repeat the author's most significant findings. The analytical table of contents (pp. 1211–22) serves as the best guide to specific subjects treated in this informative and unique work.

HERBERT MOLLER
Boston University

GÜNTER BARUDIO. *Absolutismus—Zerstörung der "Libertären Verfassung": Studien zur "Karolinischen Eingewalt" in Schweden zwischen 1680 und 1693*. (Frankfurter Historische Abhandlungen, number 13.) Wiesbaden: Franz Steiner Verlag. 1976. Pp. xi, 248. DM 64.

The renewed interest in the origins of European absolutism represented in this work was stimulated by the revival of parliamentary government in West Germany after the Second World War. The new generation of German historians has at least begun to produce an independent perspective on the long-dominant Prussian or "Borussican" historiography of absolutism. Rooted in the mentality of Prussian royalist apologists bred during the Bismarck era, it promoted the view that any defense of constitutional rights on the part of early

modern parliaments could be regarded as mere expressions of "wicked" self-interest, while the policy of the crown was usually considered to be "progressive" and necessary for the making of the modern state. Günter Barudio attacks the premises of the Borussian school and seeks to delineate more clearly the libertarian (proto-liberal) political theories which emerged in Sweden at the end of the seventeenth century. As such his work is also useful for the historians of German and Russian absolutism who must deal with similar structures.

First, Barudio rejects the prevailing dualist interpretation that the modern state developed in terms of two powers, crown and parliament (p. 9). This view merely reflects the late-nineteenth-century struggles of liberalism in the Bismarckian empire. Absolutism arises when elective kingship is replaced by hereditary monarchy, which may, however, be of two kinds—a matter usually ignored by early modern historians. Barudio distinguishes patrimonial and emphyteutic hereditary kingship, especially in the Swedish case. Of course, Viking practice had been to consider government as a kind of property. By the end of the seventeenth century when Charles XI did succeed in establishing *Enväldde* or unified authority (absolutism), the constitutional arguments used by aristocracy and crown made clear a distinction of the crown as subject to public law only and exempt from private (civil) law. The royal domains fell under public law while the sovereignty of the crown, however, included the concept of usufruct (*dominium utile*) plus full control (*dominium directum*) of this now public property fully under royal authority and no longer requiring the consent of the estates or the privy council. Barudio sees clearly that Northern and Central European constitutions are tripartite and not dualist, that the privy council as well as the estates are able to check the crown. The break in the observance of this tradition, which occurred in seventeenth-century Sweden (and in eighteenth-century Germany), produced justifications for personalist absolutism (*l'état, c'est moi*). Only the crown represented the public interest and the estates were alleged to be incapable of rising above self-interest, an argument they sought unsuccessfully to refute. The crown could now dispose over the public good without being bound by private (civil) law, but in effect treated it as if it were a private good (patrimony). Further, the crown alleged that it was by reason of its successful conquest by arms (especially under Gustavus Adolphus) that it acquired full control and disposition (sovereignty) over its lands—a Hobbesian view at best. Even opponents of absolutism like Claes Rålamb agreed that since the king was the ultimate source of justice, he could intervene and suspend ordinary due process

for the purpose of remedying defects. By 1689 the king's marshall extended this argument to maintain that civil law did not bind the crown in its supreme power to effect justice and hear grievances. The result was that government by Will replaced government by constitution. The judgment which suspended law was applied in the name of *raison d'état*, and undermined the sovereignty of the individual, a characteristic of Swedish as well as of Prussian absolutism. Barudio's thorough use of difficult archival material and insightful reinterpretation of ideas makes this an important new contribution to the study of the praxis of absolutism and to the theory of the modern state.

HELEN P. LIEBEL
University of Alberta

SVEN G. TRULSSON. *British and Swedish Policies and Strategies in the Baltic after the Peace of Tilsit in 1807: A Study of Decision-Making*. (Bibliotheca Historica Lundensis, number 40.) Lund: CWK Gleerup, 1976. Pp. 175. KR 90.

This book presents a skillful analysis of the decision-making processes involved in the formation of British and Swedish foreign policy in the nine months after the Peace of Tilsit. Sven G. Trulsson challenges the traditional view of King Gustavus Adolphus IV as an unstable, ill-equipped monarch whose foreign policy lacked both concrete goals and direction. He offers, instead, a view of Gustavus Adolphus as a skilled manipulator of events who is intent upon expansion and prepared to exploit the strategic and political situation in the Baltic to Sweden's benefit.

An impressive array of facts has been assembled to support the author's thesis. While there are obvious, verifiable evidences of the Swedish king's mental paralysis, his overall direction of foreign affairs is characterized by calculated actions rather than hasty reactions; the extent of his personal involvement in both the development and the implementation of the intricate details of foreign policy is hardly the work of a disoriented madman. The fact that Sweden could have quite easily detached itself from Britain during the last months of 1807 or the first weeks of 1808 and yet did not gives further support to the contention that Gustavus was pursuing a conscious course of action.

Trulsson is also to be commended for his thorough use of both primary and secondary source materials. In an era when British cabinet minutes were not recorded and the Swedish king often conducted foreign affairs without consultation with his foreign minister, the task of reconstructing past events is no small matter. Printed source collections, public and private manuscript sources,

inter- and intra-governmental correspondence, as well as a large number of secondary sources were examined.

While this volume may not appeal to a very large reading audience, Trulsson is to be applauded for his thorough research and accurate presentation. His success should give encouragement to other historians to examine the much-neglected field of Scandinavian history.

RONALD L. TAYLOR
Ursuline College

KARL ENGLUND. *Arbetsförsäkringsfrågan i svensk politik, 1884-1901* [Workmen's Insurance in Swedish Politics, 1884-1901]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 82.) Summary in English. Uppsala: Historiska Institutionen vid Uppsala Universitet, distributed by Almqvist and Wiksell International, Stockholm. 1976. Pp. 207.

According to the author of this Uppsala University dissertation, seventeen years of legislative activity produced only two workmen's insurance laws and one royal decree establishing a fund for the same. One purpose of his study is to determine "the causes of this discrepancy between governmental policy and parliamentary action." A second purpose is to get behind the formal recommendations and decisions of the legislature and the ministry in order to find out "how the decisions were really made."

The worker's insurance measures analyzed here are old-age pensions, job safety measures, and health, accident, and disability insurance. Actually four bills were passed as a result of a motion in 1884 by S. A. Hedin, Sweden's leading liberal, to study and then propose legislation for workmen's insurance: a job safety measures act (1889), government subsidy for voluntary health insurance funds (1891), disability insurance (1898), and accident insurance (1901). The job safety act was merely an updating of an earlier law. More revealing is that the last two acts were passed by parliament only because they were linked to other bills much more important to the establishment—railroad construction funds and army reform. What Karl Englund shows is that while the inspiration for worker's insurance came from Germany, the political realities there did not apply in Sweden. Parliament was dominated by industrialists, landlords, and farmers, and workmen's insurance "was as a rule unrelated to all that made [this] . . . a matter of utmost importance in Germany" (p. 186). The Socialist Party was not organized until 1889 and had elected only one member by 1897.

This is a fine piece of research and analysis. Sources, published and unpublished, include gov-

ernment documents, memoirs, diaries, private correspondence, and some periodical coverage. Tables make clear complex votes in the legislative process. The bibliography and name index are good, and a four-page summary in English condenses the book down to the essentials. Happily, the reader is spared a long discourse on methodology. Englund shows the interrelationship between Sweden and foreign trends, especially in Germany. Two Swedish insurance experts (who were also astronomers) dominated the Royal Commissions charged with making recommendations but they lacked political acumen and contacts. Finally, the author explicates in some detail "how the decisions were really made" (p. 23). Pecuniary interest of the MP's, their personality conflicts, and how all this related to the politics of the more basic tariff issue are all brought into play.

PLAYFORD V. THORSON
University of North Dakota

WILLIAM A. WILSON. *Folklore and Nationalism in Modern Finland*. Bloomington: Indiana University Press. 1976. Pp. xii, 272. \$10.00.

The title of this interesting book is unhelpful. Modernity is nowhere defined, and much of the book's content deals with premodern culture and politics. No consistent meaning is given to nationalism throughout the book. Furthermore, the author seems to have more than one purpose. His preface (p. xi) suggests that his book is the "story" of a "battle" between the Right and the Left in Finnish politics, in which struggle folklore was used as a weapon by both sides. This sounds like relatively straightforward political history, but the author is a folklorist, not a political historian. His afterword (p. 205) suggests, however, that his purpose "has been to master the story of folklore and nationalism in Finland and, where possible, to learn from it." This last possibility is not developed extensively, perhaps fortunately, since William A. Wilson does not give readers the comparative cultural and political data which would enable us, presumably, to infer which parts of the Finnish experience are applicable to non-Finnish circumstances.

On the whole, apart from the largely undrawn moral lessons, Wilson's afterword is a much better description of the actual contents of this book than is his preface, which implies that roughly equal attention will be given to right-wing and left-wing politics. This promise is not kept. Most of the book is devoted to rightist perceptions of what it is to be a Finn, and almost none, except for pages 172-81, to those of the Left. Not only is the scale unbal-

anced, but the treatment of the Left is simplistic. Wilson seems unaware, for instance, of John Hodgson's magisterial *Otto Wille Kuusinen*, and of Thomas Henrikson's seminal work on Kuusinen as a national romantic. Wilson's only passing mention of the complex history of Finnish Communism is in his account of the Winter War. Edward Gylling, the interwar spokesman for a "Finnish" Soviet Karelia, gets even shorter—and less fair—shrift.

A more accurate description of Wilson's book would be that it is a study of the impact of a single literary work, Elias Lönnrot's world-famous *Kalevala*, first published in 1835–36, upon right-wing Finnish politics. The *Kalevala* was, of course, Lönnrot's highly personal ordering of some, but not all, of the many songs he had heard sung in East—i.e., Russian—Karelia, and therefore not folklore in any strict sense. The lovely, if pagan and bawdy, songs of the *Kalevala* were based on the myths of premodern Russian subjects, not on Finnish historical experiences. Wilson's real achievement is to relate, essentially accurately, how some Finns came to believe that the *Kalevala* told of the essential Finnish historical experiences, and how the last of those Finns acted, in the Second World War, to integrate Soviet Karelia into Finland to safeguard what they saw as the holy land—whether cradle, or grave, or both—of Finnish nationhood. This story may by now be essentially derivative, but to have it all in one place in English is most desirable. Seldom have art and politics meshed so completely. It is therefore unfortunate that the highest achievements of Karelianism in Finnish art—Eino Leino's lyric poetry, Akseli Gallen-Kallela's paintings, and Jean Sibelius' "Kullervo" symphony—get only passing mention, as does the work of Finland's greatest cultural historian, Yrjö Hirn.

MARVIN RINTALA
Boston College

HANS H. GERTH. *Bürgerliche Intelligenz um 1800: Zur Soziologie des deutschen Frühliberalismus*. Forward and bibliography by ULRICH HERRMANN. (Kritische Studien zur Geschichtswissenschaft, number 19.) Göttingen: Vandenhoeck and Ruprecht. 1976. Pp. 155.

The publication of Hans Gerth's *Bürgerliche Intelligenz um 1800* (written in 1935) has long been overdue. It is the best short study extant on the socio-intellectual background of the German Enlightenment. Gerth applies the sociology of knowledge to a specific historical problem: why did a large sector of the German intelligentsia accept liberalism at the beginning of the nineteenth century. Gerth treats liberalism as a specific form of

thought; it has its own distinct morphology and is explicable by reference to the social conditions in which it was generated. But he refuses to establish simplistic causal relations between social class and ideology; liberalism is not seen as a product of a specific class. In fact, Gerth argues that liberal ideas preceded the creation of a modern German bourgeoisie. The real problem was to investigate the constellation of socio-intellectual forces that coalesced into the formation of early German liberalism.

Gerth argues that two distinct streams combined to form German liberalism. The first represented the economic interests of those sectors of German society held in check by political mercantilism, namely the East Prussian nobility, the nascent capitalists, and the manufacturers who strove to implement liberal economic policies. But these groups did not constitute a bourgeois-liberal class in the English or French sense; at most they formed a liberalism of specific economic interests. The real creators of German liberalism came from the academically trained intelligentsia. The bulk of Gerth's work discusses those elements of the intelligentsia's experience and background that opened avenues for the formation of liberal attitudes. Gerth reconstructs the intelligentsia's social and experiential world with impressive deftness. He emphasizes the importance of the Protestant pastorage for providing liberal thinkers; the role of the university as the center of modern thought and as the best avenue for social advancement; the development of new forms of association, especially the new student fraternal groups and the rise of the cult of friendship; and the early vocational experience as private tutor and the role this experience had on the formation of modern pedagogy. Gerth concludes his study by examining the two areas where the intelligentsia had the most effect: the creation of public opinion and the bureaucracy.

PETER HANNS REILL
University of California,
Los Angeles

JOHN L. SNELL and HANS A. SCHMITT. *The Democratic Movement in Germany, 1789–1914*. (James Sprunt Studies in History and Political Science, volume 55.) Chapel Hill: University of North Carolina Press. 1976. Pp. x, 501. \$13.95.

At the time of John L. Snell's unexpected death in 1972 at the age of forty-eight, he had been working intermittently for several years on a major study of German democracy in the nineteenth and twentieth centuries. Since the project had aroused considerable interest among students of Central

Europe, his fatal illness appeared to deprive the historical profession not only of a prominent scholar, but of an important book as well. It is therefore pleasant to report that the work has now been brought to a conclusion, at least down to 1914, by a gifted and sensitive collaborator. Hans A. Schmitt, relying on Snell's research which was largely completed, and incorporating the chapters he had written amounting to over half of the entire volume, has done his job with sound judgment. It is a tribute to his skill that only those closely familiar with their styles will be able to tell where one author ends and the other begins. All readers, moreover, will find the narrative lively, forceful, and enlightening. This is a fitting memorial to a talented historian who suffered an untimely death.

The book does not trace the development of the democratic movement in Germany through any single faction or school. It seeks rather to re-examine the entire past of the country from the French Revolution to the First World War for evidences of libertarian thought and deed. All the familiar themes of German political, social, and even diplomatic history appear in these pages, scrutinized for what they can tell us about the struggle for freedom. The emphasis rests primarily on the groups which formed the Progressive Party, but the Social Democrats receive almost as much attention, for the authors regard them—or at least the moderate majority among them—as an important element in the democratic tradition. The book does not hesitate, however, to depart from party labels in the search for manifestations of liberty in Central Europe. Some of the most interesting sections deal with the movement for the emancipation of women, the opposition to war, and the democratization of the suffrage. The scholarly apparatus, moreover, with voluminous footnotes and rich bibliography, provides a highly useful guide to recent research. The work which has gone into this volume is prodigious.

As for the conclusion, there is no formal statement of findings. Snell did not live long enough to formulate one, and Schmitt wisely decided that it would be best not to try to summarize in his own words what his predecessor had meant. Still, there can be little doubt about how they feel concerning the development of the democratic movement in Germany. They reject the view that there is a profound historical and ideological gulf separating Central from Western Europe. The significant differences here are of degree, not kind. Writing about the Revolution of 1848, for example, they concede that an underlying reason for the failure of democracy was "the deeply embedded respect for traditional authority, which had been instilled in Germans for too many centuries, by too many rulers, and by too many nobles, pastors, and

teachers" (p. 124). Yet they also hasten to point out that the course of development in England or France was not much different: "The fact is that all of Europe's democratic experiments of 1848 were failures" (p. 125). The ultimate tragedy of the struggle for freedom, as they see it, was that "time ran out in 1914, before democracy on any terms had permeated German politics and society" (p. 334). Even those who perceive more serious obstacles to the triumph of liberty in Central Europe will find this book graceful, informative, and thought-provoking.

THEODORE S. HAMEROW
University of Wisconsin,

KARI HOKKANEN. *Krieg und Frieden in der politischen Tagesliteratur Deutschlands zwischen Baseler und Lunéville Frieden (1795–1801)*. (Studia Historica Jyväskyläensia, number 11.) Jyväskylä: Jyväskylän Yliopisto. 1975. Pp. viii, 288.

The title of this dense monograph by a Finnish scholar requires clarification, which he supplies early on. "War and Peace" means opinion for or against war with France, and "daily literature" means magazines and pamphlets—not newspapers—which he considers to have been too closely censored to be useful. His basic sources are fifty-eight journals (the largest of them monthlies) and some five hundred pamphlets and similar items. Most were intended for audiences restricted to one of the 300-odd minor German states. Many were for Prussian or Austrian consumption—or one or the other with her client states or allies. A few were directed to the whole of "third Germany," and fewer still circulated in Germany and Austria. None had many readers. Schiller's *Die Horen*, a large "neutral" monthly—with contributors such as Goethe, Gentz, Fichte, and Schlegel—had fewer than five hundred subscribers. The pamphlets generally came out in issues of a few dozen to a hundred. Those printed in lots of a thousand or more and sold in Germany and Austria were invariably by established authors such as Kant, Görres, Wieland, and Gentz.

The author declines to examine how or if the literature affected the thinking of the masses. The "simple folk," he says, were not interested. Neither has he undertaken to study the intellectual evolution of the arbiters of German thought in the manner of Hans Kohn, nor the budding of nationalism and romanticism à la Jacques Droz, nor the influence of Jacobinism among the masses like Joachim Streisand and his East German colleagues. He touches on these themes only to note the gradual abandonment by major intellectuals of Enlightenment ideals and cosmopolitan attitudes to serve narrower Austrian-imperial or Prussian

state interests. His hero is one who did not, however, turn pragmatist—Immanuel Kant—a dedicated pacifist who held to the principles of his “*Zum ewigen Frieden*” (1795) throughout the period and until his death.

Kari Hokkanen's contribution lies in the way his evidence highlights not only the inability of Prussia to see France as a greater enemy than Austria, but the crass parochialism of the intellectuals of the minor states. Virtually never do they see themselves as Germans. When they are for peace they argue in terms of territorial or material gain; when they opt for war they damn the French Revolution as the work of atheists, Freemasons, and (sometimes) Jews. The author concludes with a quantitative summary of opinion for or against peace in the years 1795 through 1801. In neutral Prussia and her client states writers in heavy majority are “for peace” throughout. In Austria they are “for war” until 1801, when there is no choice but peace. In “Third Germany” (the lesser states), though opinion is always mixed, writers stand with Prussia “for peace” in 1795 and 1796, are badly divided 1797 (when France proposes a radical redistribution of territories), come out for war in 1798–99, when the Second Coalition has France on the defensive, divide again in 1800, and stand for peace in 1801, those in each state hoping to curry favor with Napoleon.

Jacques Droz concluded years ago that Germans had no national spirit in 1789, but saw “patriotism” being generated by French aggression after 1793. Hokkanen's study shows that few Germans had any such feeling between 1795 and 1801; their horizons were the boundaries of their principalities.

OWEN CONNELLY AND PETER BECKER
University of South Carolina

FRANK B. TIPTON, JR. *Regional Variations in the Economic Development of Germany during the Nineteenth Century*. Middletown, Conn.: Wesleyan University Press, 1976. Pp. xiv, 270. \$22.00.

The study of industrialization by postwar German historians has tended to stress a structural approach emphasizing legal, administrative, and constitutional forms as the reflection of human acts and outlooks in the German states of the nineteenth century. More recently, however, two new influences have been added, tied in closely with the return of emigré scholars and the increased number of young historians studying in Great Britain and the United States. One is a far more critical assessment of the interaction of economics, politics, and social class, and the dysfunctional role of capitalism in German society; the other, a greatly increased, though selective, inter-

est in the tools and methods of the empirical social sciences. Frank B. Tipton's book, a revision of his Harvard thesis, continues the current trend of German historiography in this direction.

While employing “a fair amount of economic theory and quite a lot of statistics” the author also professes “a preference for explanations derived from concrete historical situations rather than from the generalized hypotheses embedded in most economic models” (p. xiii). Using the change in growth and distribution of the labor force as his prime indicator of development, Tipton finds that Germany's regions tended to diverge from each other, as well as from the national average, throughout the nineteenth century. Germany was turning from a backward country with a few advanced regions into an advanced country with important backward areas.

Tipton concludes, moreover, that the national rate and direction of change in the labor force remained rather steady. This should strengthen efforts, based on other aggregate series, to re-examine the periodization of German economic history in the nineteenth century. But the divergence between regional and national development tends to obscure, and greatly complicates the analysis of, the growth process. Not surprisingly, then, the book provides no theoretical breakthroughs (or new raw data, for that matter). Instead, it uses synthesis to correct more traditional and support more recent analyses of industrialization. Thus it confirms new interpretations of the role of political unification, migration, and even financial institutions within the German context. More generally, it points up the uncertainty of regional growth patterns in periods of rapid change, and the potential for internal violence or imperialist adventure inherent in the divergence of regional social and economic structures. And above all, it underlines the cumulative continuity of the growth process with its complex interweaving of value, class, and power conflicts. For these reasons and, last but not least, its care and moderation, this is a book which should be on every cliometrician's shelf.

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VOLKER HENTSCHEL. *Die deutschen Freihändler und der volkswirtschaftliche Kongress, 1858 bis 1885*. (Industrielle Welt: Schriftenreihe des Arbeitskreises für moderne Sozialgeschichte, volume 16.) Stuttgart: Ernst Klett Verlag, 1975. Pp. 308. DM 62.

Untangling the varied strands of German liberalism has provided historians on both sides of the Atlantic with a perennial theme. Volker Hent-

schel's effort displays the same "fascination with noble purpose" that animated members of the Congress of German Economists from its founding in 1857/58 to its slow demise in 1884/85. The Congress propagated free trade doctrines together with a panoply of measures to free the domestic market from burdensome mercantilist and guild restrictions. John Prince-Smith was its most celebrated economist. Hermann Schulze-Delitzsch moved on its periphery. Ludwig Bamberger was its most illustrious political figure.

Hentschel provides sketches of other notables in the Congress to show that their opinions were neither doctrinaire nor neglectful of the social issues besetting Germany's rapid industrialization. The journalists, Otto Wolff, Otto Michaelis, and Alexander Meyer, remained pragmatists, while Julius Faucher, H. B. Oppenheim, and Viktor Böhmert developed, in Hentschel's opinion, meaningful theories about the relation between civil liberty and the free market economy. *Kleindeutsch* views predominated among the Congress' members whose patriotic illusions nurtured confidence in "Prussia's German task." They supported Bismarck wholeheartedly during the Prussian constitutional crisis, an awkward position for liberals. With the founding of the North German Confederation (1867) the Congress attained the apogee of its public influence. Through Michaelis, who entered the *Bundeskanzleramt*, the Congress could be said to have shared in the liberal economic law-giving that issued from Delbrück's ministry.

The Congress began to falter when the German labor movement assumed a political form. It did not matter that the free-traders had long advocated labor's freedom to organize, or that they had been concerned about the industrial workers' plight. But their formula for well-being based on individual initiative and self-help limited their social range, especially when contrasted with the Socialists of the Chair. In its altercation with them, often emotional and ill-tempered, the Congress fared ill because the professors showed more virtuosity in polemics intended to form public opinion. Hentschel insists that historians have accepted the professors' diatribes uncritically, giving rise to a false view of the Congress as a monolithic block of Manchesterists.

While the Congress did sustain a modest social outlook it never overcame its readiness to consider the social question "a sum of varied misfortunes" that could be overcome by personal determination, thrift, and the moral thrust of civil liberty. The crash of 1873 embarrassed the Congress as a firm advocate of a market-directed economy. Soon thereafter the iron- and steel-makers' demand for protective tariffs confronted the Congress with resolute antagonists whose program eventually be-

came the national policy. By the eighties the Congress' sessions became lame and halting, although the free trade principle did not lapse entirely as an element in the liberal outlook: in Theodor Barth it had an eloquent advocate.

Hentschel appears to be unaware of the American publications that deal with German social and economic history. His historical method also contrasts sharply with the quest for social depth that has begun to characterize the style of the younger German historians. Hentschel concentrates on ideas and the logic that accompanies them. He omits references to interest groups as well as an analysis of the varied economic needs within the industrial and mercantile sectors. For Hentschel, the liberals' free trade views are encapsulated realities free of any connection with active economic interests or *parti pris* associations.

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MICHAEL GUGEL. *Industrieller Aufstieg und bürgerliche Herrschaft: Sozioökonomische Interessen und politische Ziele des liberalen Bürgertums in Preussen zur Zeit des Verfassungskonflikts, 1857–1867*. (Sammlung Junge Wissenschaft.) Cologne: Pahl-Rugenstein. 1975. Pp. 304. DM 48.

Michael Gugel makes important contributions to the literatures on nineteenth-century German liberalism and the peculiarities of Germany's development. He argues that historians persistently misinterpret the Prussian constitutional conflict of the 1860s when they claim that the liberals sacrificed principles and accepted Bismarck's *Realpolitik* in exchange for minor reforms and national unification. Some of the most effective passages in Gugel's book are critiques of previous literature. He demonstrates that many Marxist interpretations of the Prussian conflict utilize formulations similar to those employed by other historians. In his view the liberals effectively pursued the interests of the bourgeoisie, which had shifted since 1848. The liberals neither wanted nor needed direct control of the state. The basic clash in Prussia was between the working class and the propertied, not between the bourgeoisie and the nobility or the bourgeoisie and the state. The liberals wanted to satisfy specific requirements of an expanding industrial system, and they hoped to depoliticize most issues in order to remove them from an arena in which the populace might claim some role. If the liberals began to become doctrinaire, the explanation is that they feared the working class.

Gugel's evidence of the liberals' economic objectives is less well marshaled than his thesis is argued. Despite his reliance on archival material and

reports of chambers of commerce, we see tantalizingly little of these objectives.

The most ambitious part of the book is his attempt to demonstrate that the Prussian liberals were similar to liberals in other countries at a comparable stage of development. He argues that even in France and Britain the bourgeoisie inclined toward compromise with conservative forces, eschewed direct rule, and favored less-than-sweeping reforms. The difficulty with this part of his work is that he does not adequately exploit the secondary literature on nineteenth-century Britain and France.

Although the book is redundant in its argument and skimpy in its presentation of evidence, Gugel casts doubt on the assumption made by Barrington Moore and many others that a tendency toward liberal democracy is a usual concomitant of industrialization in bourgeois society.

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FOLKERT HAFERKORN. *Soziale Vorstellungen Heinrich von Sybels*. (Kieler Historische Studien, number 23.) Stuttgart: Ernst Klett Verlag. 1976. Pp. 222. DM 44.

How extraordinary it is to witness the publication of narrow monographs in Germany at a time when many more worthy books are turned down by publishers elsewhere! It is a marvel that *Kieler Historische Studien* can still afford to bring out such a well-produced minor study as this in 1976, for even in the best of times in academic publishing, this fruit of scholarly labor should have found its home in a journal, as a judiciously pruned article.

Folkert Haferkorn's work bears all the marks of a very competent dissertation on a somewhat marginal topic. Sybel was clearly one of the more important historians of the *kleindeutsch* school of early Imperial Germany; a number of articles and monographs on him in the last fifteen years reflect this fact. His "social conceptions," however, did not differ radically from those of other upper-middle-class, Protestant, nationalist, moderate-liberal history professors of his day: they were received and not original, as Haferkorn admits. This competent, if pedantic, essay attempts to impose theoretical rigor upon the untheoretical and rather widely dispersed views of the social order held by a mid-nineteenth-century historian. The author has worked hard to underline the nuances in thinking distinguishing Sybel from others (such as Treitschke and Schmoller); but the differences are slight in many cases.

Although Sybel's historical works and the (rather thin) *Nachlass* in Merseburg form the back-

bone of the source material, this is not an intellectual biography. Instead, the chapters are organized in conceptual terms (e.g. "Family," "People and Nation"). Given the progressive tendency of German academic historical writing in Sybel's generation to shy away from or even condemn social science in general, Haferkorn's scholarship tends to be a study in futility. It was precisely during Sybel's active life that German historiography turned away from an interest in social science and toward the biographical and purely political interest that dominated it until the turn of the twentieth century. The connections between economic, political, social, and cultural history that Sybel's own teachers dimly perceived and which his younger colleagues Schmoller and Lamprecht tried to revive became increasingly unimportant to Sybel and his *kleindeutsch* colleagues. They subsumed society into the state, their primary concept in historical interpretation. Sybel progressively lost touch with the tentative holism of his teacher Savigny.

The very absence of a cohesive social theory characterized Sybel and many other *kleindeutsch* historians; they accepted, it would seem, most of the reigning ideas of their class and time. These ideas have been analyzed elsewhere in great detail. Thus only those who have an interest in Sybel *per se* will gain further knowledge from this scholarly, well-researched, and probably unnecessary book.

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EBERHARD NAUJOKS. *Die parlamentarische Entstehung des Reichspressegesetzes in der Bismarckzeit (1848/74)*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 58.) Düsseldorf: Droste Verlag. 1975. Pp. 247.

After 1871 the newly united German Empire required a statute to supersede the varying press laws of the federal states. Eberhard Naujoks traces the evolution of the imperial Press Act of 1874 by summarizing the parliamentary and press debates and cabinet records. He did some of the research before Prussian archives were dispersed in World War II, and it is unclear whether East German archives, which were not used, hold more material on the subject.

The book—which is unfortunately too long, laboriously detailed, and positivistic—does not attempt to argue a thesis. It could have been more valuable had the author tried to explain the German liberals' attitude toward freedom of the press in the wider context of their untiring faculty to compromise with authoritarian government.

Naujoks shows that by 1871 only some journalists and a few left liberals still upheld the 1848 ideal

of a free press, subject only to criminal prosecution and trial by jury. The national liberals demanded an end to the administrative checks and economic burdens which hamstrung the press, while conceding the political need for some restraints, such as full legal liability of editors, censorship of placards, and the right to prosecute and ban foreign papers. The Prussian government and its conservative supporters wanted to retain well-trying means to silence overly zealous critics. These "preventive regulations" included licenses for printers and publishers, censorship prior to publication, special newspaper taxes, and security bonds. In 1874 the government reluctantly introduced a press bill in the Reichstag, in response to parliamentary prodding, and in order to win liberal support for an army bill. The final Press Act was a characteristic compromise: the liberals won economic concessions when newspaper security bonds and taxes were scrapped; the government could still harass the press by confiscation without legal warrant, and did so. The author does not analyze why liberals did not hold out for a free press; nor does he examine the link between the press act and the much more controversial Army Act.

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HARRY KENNETH ROSENTHAL. *German and Pole: National Conflict and Modern Myth*. Gainesville, Fla.: University Presses of Florida. 1976. Pp. viii, 175. \$10.00.

In his preface Harry Kenneth Rosenthal explains the purpose of this book as follows: "Specifically, what the author hopes to accomplish is to present a coherent argument concerning the development of the German myth of the Pole and to explore to what extent this myth made possible the Nazi slaughters in Poland during the Second World War" (p. vii). This uninspiring promise finds ample fulfillment in the book. Who can be surprised to discover a most unflattering "German myth of the Pole"? And who can see much purpose in the exercise after Rosenthal concludes that the myth was of secondary importance, inasmuch as "the Nazi slaughters" grew out of racist thinking unique to the Nazis? In short, the book focuses on the development of an attitude of little consequence, in the author's own view, to the central question he posits.

"The German myth of the Pole" is an ambiguous and disturbing concept, particularly when the author launches his search with the early Middle Ages and makes it all the way to 1894 in thirty pages; he makes little or no effort to explore variations of interest or opinion across different social classes or geographical regions of Germany. The author fixes simply on a succession of individuals

whom he considers representative of German opinion of their time, but he seldom explains why their ideas should be considered typical. And when he turns to contrast their opinions with the reality of Polish accomplishments, he prefers praise of Poland to critical analysis. Of course, Germans harbored prejudices, but that observation, taken in isolation, fails to qualify even as a curiosity.

There are other shortcomings as well. Suggestive aspects of the topic are simply ignored. One wonders, for example, how the large migrations of Poles to the Ruhr industrial area affected German attitudes toward Poles, but there is no mention of the phenomenon. Furthermore, the use of both primary and secondary sources is uneven at best. On a broad and diffuse topic such as this, the bibliography could have been expanded almost limitlessly. Selectivity was obviously essential. It is surprising not that there are omissions in the source material but that the author claims to have "read all the writings he could obtain having to do with the German image of the Pole" (p. vii).

Despite these weaknesses, the closing chapter on current German views of Poland does reflect Rosenthal's powers of contemporary observation and analysis; the view of the present is considerably stronger than the treatment of the past.

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RICHARD GUTTERIDGE. *The German Evangelical Church and the Jews, 1879-1950*. New York: Barnes and Noble. 1976. Pp. 374. \$18.50.

This excellent monograph concentrates six of its eight chapters on the *Kirchenkampf* (Church Struggle) in the Third Reich. Still, this study is the only one I know of in English that pursues this theme from the 1870s through the 1940s. The other two chapters provide an introduction to the *Kirchenkampf* by charting the preceding rise of *völkisch* thought and its attendant anti-Semitism within the Protestant churches. By regarding the *Volk* as divinely ordained, many churchmen (but not all) could deplore Jewish influence for its allegedly disintegrating impact upon the German *Volk*, whose values they hoped to see revived and infused with a Christian spirit. Thus Gutteridge makes clear that the Nazis did not create the "Jewish question" for the Protestant churches.

The Nazis exploited *völkisch* feeling and residual anti-Semitism as a means of "co-ordinating" the churches ideologically. That the Nazis and their allies, the German Christians, only partially succeeded in purging the churches of Jewish pastors and members was largely due to the Confessing Christians, who protested a racial definition of the church that would subvert traditional church doctrine. Here Richard Gutteridge traverses familiar

ground while emphasizing the centrality of the Jewish question in the *Kirchenkampf*.

Not one to slight the heroism of Confessing Christians, Gutteridge nevertheless documents the limits of their heroism beyond church doors; Confessing Christians drew an "inner line" against the Nazi regime, protesting incursions into church life while declaring their loyalty to the point of taking an oath to Hitler. Again, this point is a familiar one. But Gutteridge demonstrates the rigidity of that line with respect to Jews. With notable exceptions, Confessing Christians did not pass beyond this line to contest Nazi measures against Jews. In his concluding chapter, Gutteridge has written an excellent analysis of the reasons for this line of no retreat and little advance.

The book does well in elaborating shifting positions, citing examples, and offering explanations. But the underlying conditions to which these refer are only weakly sketched in. The reader must long to know more about what actually happened to Jews within these churches and, given the inconclusive struggle, the extent that measures inspired by Nazis and German Christians found implementation. The outlines of the *Kirchenkampf* emerge only dimly. Yet the condition of Jews must have varied with each successive stage in that struggle and within each of many Protestant churches (as the author sometimes indicates). Still I commend the author for condensing the results of wide reading into a well-developed monograph. The study makes conceptual sense of a theme that spans many years and that has often frustrated our understanding.

DANIEL R. BORG
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RUDOLF KALLNER. *Herzl und Rathenau: Wege jüdischer Existenz an der Wende des 20. Jahrhunderts*. Stuttgart: Ernst Klett Verlag. 1976. Pp. 446. DM 39.

Theodor Herzl, the Austrian journalist and founder of political Zionism, and Walther Rathenau, the German industrialist and Foreign Minister of the Weimar Republic, provide the subjects of this book. To Rudolf Kallner they are primarily of interest as prominent representatives of contrasting modes of Jewish existence: Zionism and assimilationism. Actually, Rathenau is included merely as a counterpoint to Herzl with whom Kallner's sympathies clearly rest.

A sketch of nineteenth-century attitudes toward the Jewish question provides the background for an account of the divergent solutions proposed by Herzl and Rathenau. Noting that the assimilated young Herzl had little liking for Jews, his transformation into a tireless champion of Jewish statehood is attributed to the anti-Semitic atmosphere

of the closing decades of the century. No effort is made to relate Herzl's conversion to his personal and psychological problems. Rathenau, too, viewed Jews negatively but insisted the solution was primarily cultural: German Jewry must cast off those characteristics which made them disliked. As Herzl rightly observed in one of the few letters the two men exchanged, they shared the same premise, but had arrived at different conclusions; unfortunately, Kallner casts little light on the reasons for those differences. To cite Rathenau's scant knowledge of Jewish history and life is of little value, since the same can be said of Herzl.

The longest portion of the book traces Herzl's organizational and diplomatic activities without breaking any new ground. A concluding section follows the evolution of Rathenau's attitude, through his assassination in 1922. Admitting he gradually developed a more positive assessment of Jewish qualities, spoke out more forcefully against the injustice accorded German Jewry, and indicated an interest in visiting Palestine, Kallner rightly concludes Rathenau remained committed to the concept of assimilation.

Based upon published sources, the book adds little to what is currently known about either man. Kallner's conclusions are particularly lame. It scarcely justifies a full length book to note that Herzl is a national hero in Israel while Rathenau is all but forgotten in Germany.

A broader approach might have produced more illuminating results. Psychologically, the two had much in common: unusually close maternal ties, difficulty in developing satisfactory personal relationships, and a desire to dominate man and situations. Socially, they shared an admiration for the Prussian aristocracy, the blond, blue-eyed Germanic type, and a negative assessment of contemporary Jewry. Recognizing the need to recast liberal institutions, they envisaged utopian communities emphasizing social justice and modern technology. Rathenau eventually longed for a Germany of mixed population where the question of one's ethnic origin would never be raised. Herzl dreamed of a cosmopolitan Jewish state which his critics faulted for its lack of Jewish character. The differences between Theodor Herzl and Walther Rathenau were significant and real, but in ignoring or failing to elaborate their many similarities, Kallner has seriously diminished the value of his book.

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GERHARD EBEL and MICHAEL BEHNEN, editors. *Botschafter Paul Graf von Hatzfeldt: Nachgelassene Papiere, 1838-1901*. In two volumes. (Deutsche Geschichtsquellen des 19. und 20. Jahrhunderts, volume 51.)

Boppard am Rhein: Harald Boldt Verlag. 1976. Pp. lxxv, 672; 673-1433.

Count Paul von Hatzfeldt corresponded with some of the most important participants in German foreign affairs during his tenure in the diplomatic service. His letters, as well as those to him from such men as Herbert Bismarck, Radolin, Bülow, and especially the almost four hundred from Baron von Holstein, are included in these two volumes. These selections have been collected from his private papers and numerous other archival sources. From the early days of Hatzfeldt's career when he served as an unpaid attaché at the Prussian Embassy in Paris to his sixteen-year stint as German ambassador in London, he was at the center of German diplomatic activity.

Wherever he was stationed, Hatzfeldt made friends. He seemed to have the ability to attract the attention and solicitude of those in positions of power; while in Paris, for example, he caught the attention and friendship of the Empress Eugénie and also of Bismarck, who would later say that Hatzfeldt was "the best horse in my diplomatic stable." At the same time, his relations with Holstein were close. Not only did Holstein serve as Hatzfeldt's mentor, but Hatzfeldt influenced the baron. As Holstein pointed out in November 1893, "I often have the feeling that for ten years I have worked as your political undersecretary." Their correspondence is filled with detail about the aims and methods of their diplomacy.

Hatzfeldt's posts never seemed dull. When he was first sent to Madrid in 1874, Spain was a republic and he was there when the monarchy was restored. In 1879 he became ambassador at Constantinople where he played a major role in the aftermath of the Berlin congress. For his able handling of German interests William I awarded him the Red Eagle, first class, one of many decorations he would receive. Next Hatzfeldt became secretary of state in the Foreign Office, a post he held until he went to London as ambassador. Throughout his career the Count was plagued by financial difficulties and his correspondence with Bleichröder, the Berlin financier, is revealing of the relations between financial and diplomatic circles.

In addition to letters the collection also contains a fragment of a memoir Hatzfeldt began in 1892 that discusses the early part of his career. During the Franco-Prussian War he was a member of Bismarck's entourage at his field headquarters. Hatzfeldt's facility with the French language gave him unusual opportunities for so young a diplomat and enabled him to observe Bismarck closely; in his recollections the count gives his evaluation of Bismarckian policies. For these reasons the historian interested in the diplomacy of the late nine-

teenth century would find this collection useful even if it were not for the wealth of material—over nine hundred pages—that is devoted to his London post.

The letters from the early period of Hatzfeldt's life are also of interest. He was the youngest child of a marriage of convenience that ended in divorce, and the controversy attracted the attention of Berlin society in the middle of the century. Ferdinand Lassalle became his mother's lawyer and champion, while their relationship earned her the title "the red Countess." Early in his youth Hatzfeldt was a partisan of Lassalle, but by December 1851 he wrote to his mother that he had forsaken socialist and democratic ideas "to return to the principle of legitimacy." The letters between mother and son demonstrate the difficulties the youthful involvement with Lassallean ideas created for young Hatzfeldt. There is also information in the countess' letters to her son about her relations with Lassalle, the difficulties of her position as a divorcee without independent means, and the methods of obtaining preferment in state service.

The editor has done an excellent job of selecting and annotating the collection and has provided a first-rate introduction. The work is a valuable contribution to the already immense amount of material about the diplomacy of the first three decades of the German Empire.

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LAMAR CECIL. *The German Diplomatic Service, 1871-1914*. Princeton, N.J.: Princeton University Press. 1976. Pp. xiv, 352. \$20.00.

This work offers an analysis of "diplomats, not . . . diplomacy." The author describes the structure of the Foreign Office, conditions of entry, the role of the military, career patterns, and finally, the labors of diplomats under three emperors and five state secretaries. Lamar Cecil's conclusions are hardly surprising: of the 548 diplomats investigated, 69% bore titles of nobility, almost one-third possessed the *doctor juris* degree, 20% were fraternity men, and 82% were Protestant, 17% Catholic, and 1% Jewish. Moreover, nearly one position in every ten fell to army officers, while the bourgeois element in the diplomatic service increased from 8% in the 1870s to 42% by the 1900s; newly entitled nobility accounted for a further 18% of the Wilhelmstrasse's staff. Nor can one argue with the author's statement that advancement was "very often related less to a diplomat's capacities for negotiation than it was to his birth, his command of riches, his sociability, or the tastes and attractiveness of his wife" (p. 188). Cecil claims

that the Foreign Office was to the Prussian nobility "a family operation," but adds that the *Adel* "did not have any demonstrable effect on policy" owing to the dominating personalities of Bismarck and Wilhelm II. Various recent studies dealing with other professional groups in Wilhelmian Germany have revealed similar findings and have stressed the crucial roles of birth, wealth, religion, and education in that society. Unfortunately, Cecil does not cite these works.

The book's attempted thesis concerns the decline of Prussian aristocrats in the foreign service between 1871 and 1900 owing to "the agricultural depression which became perceptible in the early 1870s and critical by the beginning of the following decade" (p. 177). Cecil links the decline in grain prices over these years directly to the "marked" decline (7.02%) in Prussian aristocrats at the Wilhelmstrasse—as well as to the "recovery" (1.14%) after 1900. However, his assertion that most nobles were "associated" or "connected" with the land does not sufficiently delineate the diplomats' financial dependence upon land. Nor does he convincingly link the agricultural crisis to an inability by the Prussian aristocrats to provide the annual income of 15,000 marks required by the Foreign Office for entry. And are there not other reasons for the decline in Prussian nobles in the diplomatic corps? We know from the works of Demeter, Fahlbek, Kitchen, Gruner, Rumschöttel, and others that the overall number of nobles in the other German states as well as in Sweden fell precipitously in the nineteenth century: in Bavaria, for example, from 1384 families in 1822 to 715 in 1918. In short, this highly challenging aspect of the book deserves further intensive investigation.

Cecil's work is based upon approximately twenty *Nachlässe* deposited in various German public and private archives as well as a vast pre-1945 memoir literature. There are few books from the last thirty years cited in the footnotes and there is no bibliography. The myriad tables are at times overwhelming, and the given percentages do not always add up to one hundred. The few pages appended at the end concerning the role of the Reichstag in diplomatic affairs are sketchy; they might have been deleted or the subject given separate treatment, as was that of the army.

The author's conclusion that the *Auswärtiges Amt* in 1914 was "sprawled in immutable repose" and watched the world go by "with a weary and disapproving eye" would arouse comment, to say the least, from Fischer, Geiss, Berghahn, Röhl, *et al.* Yet, on the whole the work deserves consideration in future investigations of Wilhelmian society.

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IMANUEL GEISS. *German Foreign Policy, 1871-1914*. (Routledge Direct Editions.) Boston: Routledge and Kegan Paul. 1976. Pp. ix, 259. \$13.50.

Immanuel Geiss, one of the earliest and most accomplished of Fritz Fischer's disciples, appropriately has dedicated his survey of imperial German foreign policy to the master. In both its argument and its polemically insistent tone Geiss' work, which is based on familiar printed materials, bears some of the hallmarks of the Fischer school. Germany is held to be overwhelmingly responsible for the incubation of the First World War, the decision to provoke the conflict having been taken late in 1912. Germany's role in causing the war was indeed, to Geiss, the inexorable result of a foreign policy which for decades before 1914 had been "hectic, boundless and uncontrollable" (p. 16). Its diplomatic maneuvers were the external solution which the elite devised to deal with internal problems threatening the continued existence of the empire.

This book is of interest because it is crisply and vigorously written and because it is the most recent Fischerite examination of the broad course of imperial foreign policy. Those in sympathy or agreement with Fischer will, however, be wary of employing this work to fortify their position. Contradictions, such as whether Russia or Germany was basically responsible for inaugurating the system of alliances, need to be resolved, and questionable arguments—among them the charge that the German shipping industry was an element in the opposition to a reconciliation with England—need reappraisals which probably would result in their being discarded. The essential difficulty in Geiss' treatment resides, however, in what he announces as his "basic idea" (p. vii), which is to demonstrate the interdependence of domestic and foreign policy in imperial Germany. This he believes can be accomplished by studying the alternatives which presented themselves to Germany's leaders and by analyzing the pressures brought to bear on them by public opinion or by broader historical forces. Geiss declares at the outset of his study that he will eschew "elaborate psychological portraits" (p. vii); indeed, he attempts not even the most rudimentary character analysis of the men who forged Germany's diplomacy. Bismarck's personality, we are unconvincingly assured, though not "irrelevant" is only of "secondary importance" (p. 44) because he was powerless before "dominating trends," while William II, who presents a more demonic problem, is accorded a "considerable amount of influence in the formulation and execution of German foreign policy" (p. 63).

Geiss' declarations to the contrary, his narrative is actually a traditional account in which foreign policy is shown to be based on external consid-

erations often virtually independent of domestic considerations. Moreover, the personality factors which he avoids are by his own reckoning often of primary importance. A good example of both is the treatment Geiss gives of the 1879 alliance with Austria-Hungary, where diplomatic motives and Bismarck's mood determined the course of events. Geiss is surely correct in reserving to William II a large role in German diplomacy, but he fails to appreciate that the Kaiser's temperament was far too erratic to be able to engage in the systematic pursuit of an internal or diplomatic goal, much less to understand any policy that sought to combine the two. William II's responses were almost always highly personal reactions to challenges, real or imagined, to his person. The catalyst for the emperor's decision in 1896 to build a great navy, as Geiss correctly argues, was the Krüger telegram imbroglio. William II's motivation was not, however, the pursuit of a new *Weltpolitik* which would arrest undesirable internal development while strengthening Germany abroad, but rather a desire to put England on notice that he was not to be trifled with. Geiss' characterization of the Kaiser tends to impute to him a more pervasive influence than he really enjoyed, for the historian seems not to understand what contemporaries fully realized: most of William's verbiage was mere theater, and, while he had to be obeyed, he did not have to be taken seriously. The Kaiser's antic utterances might lead to a flurry of memoranda, but they are not necessarily signals that German policy had acquired a new coloration. Geiss' tendency to give undue weight to William II's menacing rodomontades is compounded by his literal acceptance of Bülow's unctuous imitations of his sovereign's ravings. He argues that the chancellor's observation to the Kaiser in 1909 that "we . . . will throw many corpses at the feet of our enemies" is evidence of a "kind of 'Götterdämmerung' atmosphere" (p. 110) is misleading. It was merely a fulmination by the "eel" calculated to titillate the bombastic, ever impressionable ruler, who, as Geiss aptly observes, was lamentably the true symbol of his unfortunate empire.

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WILHELM DEIST. *Flottenpolitik und Flottenpropaganda: Das Nachrichtenbureau des Reichsmarineamtes, 1897-1914*. (Beiträge zur Militär- und Kriegsgeschichte, number 17.) Stuttgart: Deutsche Verlags-Anstalt. 1976. Pp. 344. DM 48.

Admiral Alfred Tirpitz has been referred to as the "first propaganda minister of modern stamp," and indeed the construction of the German battle fleet

was accompanied by a public relations campaign of previously unknown dimensions. The instrument for the coordination and dissemination of information to influence public and political opinion was the *Nachrichtenbureau* (News or Information Bureau) of Tirpitz's RMA (Imperial Naval Office). Wilhelm Deist, a research consultant at the *Militär-geschichtliches Forschungsamt* in Freiburg, a co-editor of its journal, and author of numerous publications on the Imperial German Navy, is well qualified to investigate the functions of this little-known organization; for while the activities of the *Nachrichtenbureau* (NB) have been reflected in other sources, this is the first work to rely extensively on the Bureau's records themselves in the Military Archives at Freiburg.

Tirpitz had learned the significance of public relations as Chief of Staff in the OKM (High Command of the Navy). His first campaign, however, was to champion the Mahanist concept of a battle fleet vis-à-vis the prevailing cruiser-warfare strategy, within the navy itself. When Tirpitz became state secretary of the RMA in June 1897 he immediately created the *Nachrichten-Abteilung* as department M II under his trusted subordinate, Lieutenant Commander August von Heeringen, who played an active role in the passage of the 1898 and 1900 Navy Laws. Yet it becomes evident that the Bureau was very much the creation of Tirpitz himself. The successive heads of the NB, usually of lieutenant commander to captain rank, obediently disseminated the information necessary to influence passage of the Navy bills drafted by Admirals Eduard Capelle and then Harald Dähnhardt of the RMA Central Department. Its greatest challenge, in fact, was countering the damaging demagogery of the Navy League, which threatened to alienate the Catholic Center, a party Tirpitz believed a necessary political ally. The *Flottenverein*, formed by business interests to encourage expansion abroad, was always an uneasy ally at best because Tirpitz, building a battle fleet, was opposed to overseas naval commitments; engineering the removal of retired Brigadier General August Keim as the League's director in 1908 was a major triumph.

The routine activities of the NB seem to have been supporting books, brochures, newspaper articles, and lecture honorariums; it does not seem to have been a conduit for more nefarious undertakings. I would like, in fact, to see more discussion of specific journalists and Reichstag members contacted, both as to means and results, and contributions by business groups. It is questionable to suppose that "interested private industries" supported the naval propaganda "with extraordinarily high sums" (p. 81) without concrete documentation; but then Deist's research

was concentrated exclusively in the Navy archives.

The NB is represented as Tirpitz's instrument in defeating British overtures regarding arms limitations, especially in leaking the 1912 Novelle to the press on the eve of the Haldane mission. As a result, the chancellor and the Foreign Office both tried to dissolve the Bureau. They were not successful, though the current director was transferred to a ship command. The episode reinforces Deist's conclusion that the fleet propaganda consciously created a public opinion of Anglophobia. This conclusion, however, as well as his unsympathetic interpretation of Tirpitz's policies in general, may be debated. Nonetheless, an investigation of the RMA *Nachrichtenzentrale* gives valuable insight into the methods and policies of Admiral Tirpitz, and Deist's thoroughly researched work is a significant addition to the bibliography of this much-disputed subject.

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HOLGER H. HERWIG. *Politics of Frustration: The United States in German Naval Planning, 1889-1941*. Boston: Little, Brown and Company. 1976. Pp. x, 323. \$18.00.

Franz Halder's caustic comment in June 1942 that "these people [German admirals] dream in continents" represents the major theme of Holger H. Herwig's most recent publication. By studying the navy during three periods in German-American naval relations—the era of colonial competition 1889-1905; World War I, 1917-18; and the National Socialist period to 1941, ending in Hitler's declaration of war against the United States—Herwig demonstrates a pattern of important themes and conflict in German naval planning. The author documents the continuity of German naval ambitions (and frustrations) with the high level of scholarship that one has come to associate with his earlier works such as *The German Naval Officer Corps: A Social and Political History, 1890-1918* (1973). The most important contribution of this book, however, is less a general analysis of "continuity" than his examination of the degree to which the United States constituted a significant factor in the strategic deliberations of the German navy.

Although front-page news in 1971 when first brought to light by an article published by Herwig and David F. Trask in *Militärgeschichtliche Mitteilungen*, the existence of a German contingency war plan to invade the United States around the turn of the century, reflected the imperialist, social and economic Darwinist "spirit of the age" that characterized not only the German but also the American leaders and public. Such thinking led directly

to the creation of Operations Plan III (1903-06) against the United States and the American Black War Plan (1913-14) against Germany. From the Wilhelmian era to the Third Reich, argues Herwig, the German leaders regarded war with the United States, an inseparable ally of England, as inevitable and necessary if Germany was to survive and share in the redistribution of territories. German naval strategy in each phase was based upon exaggerated notions of American incompetence and social (later racial) inferiority, compounded by an inability (or unwillingness) to consider United States industrial or military potential. Thus, in 1941, impatient with the Führer's neglect of the navy's Atlantic U-boat campaign in favor of the Eastern front, the admirals pressed for war with the United States—another example of the admirals' traditional interference in political matters.

Herwig has much to say concerning Germany and the navy's repeated bids for *Weltmacht* and scholars will find useful his examination of the parallels in strategic and tactical concepts (e.g. *Stützpunktpolitik*, "Mahanian" battle fleet) during the different periods of German naval history as well as the navy's "politics" and the refusal of its leaders to abandon their goals in spite of political obstacles or military realities.

In summary, Herwig's thoughtful and well-researched *essai* should stimulate further inquiries. In the interim, the recent publication of volume two of Michael Salewski's *Die deutsche Seekriegsleitung, 1935-1945* and Werner Rahn's *Verteidigungskonzeption und Reichsmarine in der Weimarer Republik*, as well as Jost Dülffer's *Weimar, Hitler und die Marine: Reichspolitik und Flottenbau, 1920-1939* (1973), indicate the in-depth investigations which this reviewer feels will substantiate the "framework" provided by Herwig.

KEITH W. BIRD
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MARTIN SCHUMACHER. *Wahlen und Abstimmungen, 1918-1933: Eine Bibliographie zur Statistik und Analyse der politischen Wahlen in der Weimarer Republik*. (Bibliographien zur Geschichte des Parlamentarismus und der politischen Parteien, number 7.) Düsseldorf: Droste Verlag. 1976. Pp. 155.

This bibliography lists virtually all extant published election materials of the government statistical offices of Weimar Germany and membership lists of the parliaments and parliamentary handbooks, as well as "selected literature on the sociology and analysis of the elections" (p. 8). It does not include general reference works, memoirs, diaries, election results contained in the government press, or works on individual parties. Its

purview of periodical literature is generally limited to "the major political currents and the large scholarly organs of the Weimar Republic" (p. 9). Given the volume of foreign comment on Weimar elections, this lack of references to foreign language literature is a serious omission: of the almost 2,000 entries, only forty-nine are in English, four are in French, and four in Polish.

Nevertheless, anyone doing research on political life in Weimar Germany will find this bibliography a useful tool. It provides an overview of almost all aspects of Weimar elections including such topics as women voters, minorities, non-voters, and election propaganda. The chronological arrangement used throughout all but the final section facilitates access except in the first chapter—*Allgemeiner Überblick*—where it confuses matters. One would hope that *Wahlen und Abstimmungen* will be revised and expanded periodically so as to maintain and enhance its usefulness.

FRED A. HELMS

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CHRISTOPH M. KIMMICH. *Germany and the League of Nations*. Chicago: University of Chicago Press. 1976. Pp. x, 266. \$17.50.

Scholars familiar with Christoph M. Kimmich have come to recognize his work by the elegance and grace of his clear style. Kimmich does not disappoint his readers in *Germany and the League of Nations*, his interpretive history of German League of Nations policy, 1918–33. The book, its jacket contends, will "stimulate further debate on the behavior of Germany between the two wars." Indeed it should.

Giving half his narrative to events preceding entry and half to events between 1926 and 1933, Kimmich first surveys German attitudes toward an international organization, its non-member status, and its efforts at entry; he then interprets German policy as a League member on such issues as the Rhineland, reparations, the minorities problem, equality of rights, and withdrawal from the League. Kimmich's strongest chapters are his earliest. There he contributes to our understanding of German pacifism and internationalism in the late war years and early years of the League. But as he approaches German entry, he increases his dependence on the works of Spenz, Bretton, Turner, Gatzke, Fink, Jacobson, and Post.

Equally, a dependence on printed documents and the familiar files of the German foreign minister and state secretaries affords Kimmich little latitude to depart from established interpretations. This is demonstrated in the central position Stresemann occupies in Kimmich's account, as well as the predominance given to private diplo-

macy of the Locarnites in Geneva. It is debatable, for instance, whether the Rhineland and reparations, questions clearly outside the League, can be lumped with League policy. Kimmich's concentration on high diplomacy leads him to relegate such central career diplomats as Schubert and Bülow to insignificant roles. Likewise, it leads him to neglect the entire disarmament and security nexus before 1932, which he nimbly vaults in four pages (pp. 150–53). Frustration with security negotiations in the *comité d'arbitrage* between 1927 and 1930 played a crucial part in Germany's reinterpretation of its role in the League. German failure in 1929 in both security talks and minorities policy led directly to the German offensive to reform the Secretariat in 1930. But the total collapse in late 1932 of this reform effort and consequently German League policy is obscured in Kimmich's account by his concentration on the equality of rights issue.

The book ends with a lively account of German withdrawal from the League. Kimmich's concluding remarks seek to install German League policy within the post-1870 "continuity" of German history. While Kimmich does not materially contribute to the continuity thesis, his is an eloquent plea for further research on German League policy in pursuit of the continuity thesis. In short, while he provides an interpretive survey of German League policy, a comprehensive analysis taking the unpublished riches of Geneva, Paris, and London fully into account remains to be written.

MARSHALL M. LEE
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ERNST RITTER. *Das Deutsche Ausland-Institut in Stuttgart, 1917–1945: Ein Beispiel deutscher Volkstumsarbeit zwischen den Weltkriegen*. (Frankfurter Historische Abhandlungen, number 14.) Wiesbaden: Franz Steiner Verlag. 1976. Pp. vi, 163. DM 48.

The *Deutsche Ausland-Institut* (DAI)—founded by the Stuttgart industrialist Theodor Wanner in 1917—was one of several groups formed as a result of World War I to protect Germany's threatened political, economic, and cultural position throughout the world. Ernst Ritter is to be commended for this work—a recent dissertation at Frankfurt University—which analyses the activity of the Institute during four tumultuous decades of this century.

Ritter demonstrates that the DAI enjoyed immense success during the years of the Weimar Republic as a center of both academic research regarding foreign *Deutschtum* and as an organizer of links between the Fatherland and the millions of people of German heritage living abroad. It has often been mistakenly believed that most propagandists of *Deutschtum* in Weimar were, *ipso facto*, *völkisch* and Fascist. But the author points out that

the director of the DAI, Fritz Wertheimer, was of Jewish descent, and that the members of the board were at once convinced republicans and firm supporters of the ideals of German *Volksstum*. Further, Gustav Stresemann and Paul Löbe lent their support and prestige to the Institute not only for its economic and cultural endeavors, but because of its intensive work with emigrés as well as its popular educational programs.

If the DAI was very influential in Weimar Germany, the opposite was the case during the Third Reich. Unlike the *Volksbund für Deutschum im Ausland* under Hans Steinacher—a sister institution which maintained some independence until 1937—the DAI fell to the Nazis without a battle. Viewing it as a nest of Jews, unheroic republicans, and materialist businessmen, Hitler quickly named Karl Ströhlin—an “Old Fighter” who became mayor of Stuttgart in 1933—as its new director. Ströhlin in turn appointed Richard Csaki to manage the DAI, a man whose elitist and *völkisch* ideals were in harmony with the geopolitics and racism of the Nazis.

The DAI neither sought power nor became a policy-making office during this period; all initiatives came from Berlin. The result was that the Institute continued its former work unimpeded. It did not become involved in the bitter struggle for influence in *Volksstum* affairs waged in the 1930s by Hess, Rosenberg, Bohle, Himmler, von Ribbentrop, and Goebbels. Such institutions as the DAI were of use to the regime precisely because they did not serve in any official capacity. Whereas Gauleiter Bohle's *Auslandsorganisation der NSDAP* incensed and intimidated foreign governments, the DAI could draw on good will established over many years. Its most effective contribution continued to be with the German business community at home and abroad.

Ritter bases his work on wide archival research and important interview material. Occasional lapses into factual banality do not detract from this very informative monograph.

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ROBERT A. POIS. *The Bourgeois Democrats of Weimar Germany*. (Transactions of the American Philosophical Society. New Series, volume 66, part 4.) Philadelphia: the Society. 1976. Pp. 117. \$6.00.

The Democratic Party in the Weimar Republic appears to most American observers to have been the one German party which fully accepted the democratic republic and the ideals of Western liberalism. The parallel between the eclipse of the party and the decline of the Republic has led to an identification of the two. Robert Pois provides a useful corrective to this view of the “bourgeois

Democrats.” He shows that the Democratic leadership was as influenced as any other political group to the right of the Social Democrats by such staples of German nationalist ideology as the belief in an organic, suprapolitical state, romantic idealization of the *Volk*, unrealistic desire for a People's Community, and conscious or unconscious anti-Semitism.

Pois examines the speeches and writings of twenty-eight Democrats, among them Friedrich Meinecke, Gertrud Bäumer, and Erich Koch-Weser, from their first ambiguous responses to the Republic in 1919 to their final desperate attempt to revive the dwindling party by transforming it into the *Staatspartei* and allying with the *völkisch Jungdeutsche Orden* in 1930.

Of particular interest is the revelation that these Democrats were uncomfortable with their party's image as a “Jew party” and continually sought to minimize the Jewish affiliations of its members and voters and its ties with the “Jewish” liberal press. Pois documents well such limits to Democratic tolerance; he mentions similar and more openly expressed prejudices against Catholics in several passages, but the implications of this are not explored. He describes the failure of the “bridge-building” Democrats to build effective bridges with the Social Democrats and the German People's Party, but ignores their relations with the Center Party.

Pois concludes that voters deserted the Democratic Party because of its close identification with the Republic and that the leadership recognized this fact and guided the party ever more to the right in an effort to win back the electorate. He provides no information about the nature of that vanishing electorate. The Democratic Party won 18.6 percent of the national vote in 1919. Who were these voters and what geographic regions and occupational groups did they represent? Pois' deliberate limitation of his subject to the ideology of the leading Democrats is a disappointingly thin approach to an understanding of the party.

The book contains numerous printing or proof-reading errors and is further flawed by the author's cliché-ridden style. Passages from the German, which form a large portion of the book, are awkwardly translated, distracting from—and often obscuring—their content.

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PETER D. STACHURA. *Nazi Youth in the Weimar Republic*. Introduction by PETER H. MERKL. (Studies in Comparative Politics, number 5.) Santa Barbara, Calif.: Clio Books. 1976. Pp. xix, 301.

Before 1926 it was hard to know from moment to moment which youth group ought to be regarded

as the officially sanctioned National Socialist youth movement. With the party congress of that year the problem was resolved with the designation of the Hitler Youth as official standard-bearer of the party, and recognition was accorded the activity of the Nazi youth leader in Plauen, Kurt Gruber, by making him official head of the national organization. Problems concerning relations with the SA, low membership, and insufficient party support plagued the group for years, however, and all were not resolved until after the Nazis came to power in 1933. This study focuses on the period prior to 1933, when the Hitler Youth at its height enrolled less than one percent of the youth who belonged to organizations (p. 185) and had little or no influence beyond its own ranks. The author's reason for focusing on that period seems to derive partly at least from the fact that the documentation there is most complete (p. xix); most documents for the period after 1933 were destroyed in the war.

Peter D. Stachura makes something of a case for the proletarian and "socialistic" nature of the Hitler Youth in the Weimar period and sees significance in the fact that the *Hitler Jugend* "staked a substantial claim to its own social revolutionary ethos under the leadership of Kurt Gruber, and its pertinacity in adhering to socialist ideas . . . meant that in time the group became a large thorn in Hitler's flesh The clash which the HJ's outlook promoted with Hitler and his henchman, von Schirach, in late 1931, shows that the Führer at least believed the HJ worth bothering about, if only in a negative and destructive sense" (p. 191-92). This straining for effect makes a weaker case for the topic than actually is warranted. The true importance of the Hitler Youth lies not in the conflict of 1931—the outcome of which was hardly in doubt—but in the dedication the HJ was able to inspire in some members before 1933, and above all, in the uses to which the totalitarian regime put the organization after 1933. While the most important phase of the Hitler Youth therefore comes after January 1933, the significance of the organization's role in the Third Reich fully justifies an inquiry into its earlier development. Had there been no viable Hitler Youth, it is by no means certain that Hitler would have seized upon the idea, as he belatedly did, of making it one of the most important arms of the Nazi state.

By eking out its meager existence, attracting much attention to itself, and building up an administrative apparatus of sorts, the HJ as an organization was in a position immensely to benefit from Hitler's accession to power. (The earlier Hitler Youth leaders did not themselves, however, always fall heir to the new influence and opportunities). Stachura indirectly acknowledges the im-

portance of the later period by sketching the administrative development of the organization. The main thrust of his thesis, however, is to demonstrate the heavily proletarian social composition of the Weimar HJ (a point well made) and to stress the uniqueness within the Nazi party of the socialistic components of the Hitler Youth's ideology, although the content of this ideology is, in the nature of the case, rather vaguely presented. Aside from the doubtful utility of that approach to the subject, the work's most serious shortcoming lies in the absence of any vivid depiction of the activities of the HJ and in its failure to convey much sense of what satisfactions or dissatisfactions membership in the organization might have brought the young members.

The study is thoroughly researched and based largely on archival sources, has a number of charts, and includes a good biographical dictionary with entries on two hundred leaders of the Hitler Youth. Some of these entries contain extensive and even current information. Despite certain shortcomings, Stachura's work is a useful manual and guide to one segment of the history of a neglected subject.

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TIMOTHY W. MASON. *Sozialpolitik im Dritten Reich: Arbeiterklasse und Volksgemeinschaft*. Wiesbaden: Westdeutscher Verlag. 1977. Pp. 374. DM 19.80.

This important book is a revised version of the author's introduction to his *Arbeiterklasse und Volksgemeinschaft. Dokumente und Materialien zur deutschen Arbeiterpolitik 1936-1939* (1975). There are "corrections and improvements" throughout, and the second chapter has been rewritten and expanded.

The substance of the book will be familiar to readers of Timothy W. Mason's earlier articles. After examining the impact of the "November Revolution" upon Nazi leaders, he argues that many of them concluded that "the stab in the back" cost Imperial Germany the war because the government had failed to win the workers for a prolongation of hostilities; it was therefore essential that the Third Reich win over the working class if its own war of expansion was to be successful. The difficulty lay in the fact that the task of winning labor for a major war had to be carried out by a regime which had destroyed the labor movement shortly after coming to power. It was the function of the party, the Labor Front, the propaganda apparatus, and all the paraphernalia of mass organization to overcome this difficulty: they were to wean workers away from the "illusions" of class conflict and infuse them with the idealism of the Germanic *Volksgemeinschaft* so that

they would make the necessary sacrifices. In this, the Nazis failed completely. Labor Front functionaries, the party *Gauleiter* and Goebbels' propaganda office were all intensely concerned to maintain the popularity of National Socialism. Far from supporting the regime's efforts at ever more stringent mobilization, they argued for the necessity of maintaining living standards and meeting the workers' essential material demands. The party apparatus did not oppose a war economy, but it did put serious limits upon its expansion.

If concern with popular support made the Nazis incapable of effective economic mobilization, then much of the literature on this topic appears in a new light. The supposedly conflicting strategies of Hitler and his generals, the argument that German inadequacies could have been overcome with better planning, or the thesis that Hitler did not require a more draconian mobilization of the economy because his blitzkrieg strategy made it unnecessary, are all beside the point. The Nazis knew full well that they were not prepared for a major war and that the resources available did not suffice even for a short Blitzkrieg. They simply would not face up to the political risks of imposing major sacrifices upon the populace. Hitler, on the other hand, felt that he could not draw back from war without bringing the whole National Socialist regime down. Since the conflict between political stability and preparation for war could not be resolved, Hitler drove the entire machinery toward war without resolving any of the contradictions and, therefore, without the necessary resources. Instead, he demanded impossible performances from all concerned and apparently hoped that the pressures of international crises and war would eventually enable them to impose the necessary mobilization measures. By 1939 the shortages were so acute that Hitler could no longer wait until 1942 to unleash his war, as he had originally planned. Had he done so, argues Mason, the German economy would probably have collapsed. The initial conquests (Austria, the Sudetenland, Czechoslovakia) and the first blitzkrieg (Poland) were crucial for the acquisition of raw materials and labor forces which Hitler desperately needed for the coming major war. Indeed, Hitler turned the generals' objection to war right around by arguing that precisely Germany's economic weakness required them to go to war: "...we can go on for only a few years more. Göring can confirm that. We have no other choice, we must act." Although the problem is not discussed, the study adds to the growing evidence that the Nazi regime was anything but "totalitarian" even on essential policies.

The book is based upon broad archival research and an impressive familiarity with an extensive literature. It does not always read easily, but the

argument is clear throughout. The author has a penchant for the Marxist terminology of class conflict which seems quite extraneous to his argument; management-labor relations and the regime's concern with popularity are always "class conflict." Since the concept of class conflict used in the book does not go beyond labor's normal trade union interests, it seems odd that he considers "the economic class conflict" a "structural characteristic of capitalist society" and not simply of industrial society generally. Elsewhere he states that "every modern imperialist expansion" faces the contradiction that "the interests and traditions of the working class are incompatible with the requirements of a war of conquest." That may or may not be true, but there is no attempt at a demonstration and it is not necessary to his argument. All this indicates the nature of the author's sympathies, but it does not further an otherwise sound analysis.

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RUDOLPH BINION. *Hitler among the Germans*. New York: Elsevier. 1976. Pp. xiv, 207.

The *Hitler-Welle* rolls on, but amid the popular spray sent up from recent massive works by John Toland and David Irving, Rudolph Binion has produced a serious and substantive psychobiography emphasizing the importance and the irrational foundations of Hitler's racism. In contrast to Geoffrey Barraclough, Binion believes Hitler's *petite histoire* is vital to an understanding of his power over Germany. Binion's thesis is that Hitler's anti-Semitism and notions of *Lebensraum* were the products of a traumatic reliving of his mother's death from iodoform poisoning in the course of treatment for breast cancer by Dr. Eduard Bloch in 1907, a trauma whose effects were released by his own mustard gas poisoning in 1918. The Hitler who emerged from Pasewalk was newly dynamic and charismatic, and sensitized to Germany's needs through oral aggression exaggerated by Klara's solicitude toward infant Adolf as a "replacement child." The German people in turn, Binion argues, were "susceptible to Hitler at a key psychic sore spot: the German trauma of defeat suffered in 1918" (p. xi). Hitler's career, then, was a "double track." One led to Auschwitz as he relived the trauma of his mother's death by projecting onto the Jews his guilt at having prompted Bloch's fatal ministrations and the other to Stalin-grad as he unconsciously aligned his oral demands with Germany's own re-experiencing of Ludendorff's defeat. Finally, Hitler willed utter catastrophe as "punishment due."

In one sense, Binion is the orthodox historian's psychohistorian, for he rightly rejects the simple application of psychoanalytic concepts to history. He relies instead on exhaustive research into primary material as with his earlier investigations of Lou Andreas Salomé, Leopold III of Belgium, and Crown Prince Rudolf of Austria. His bibliography lists only one psychoanalytic secondary source: Freud's *Gesammelte Werke*. But, as Peter Loewenberg has pointed out, Binion fails to recognize intrapsychic fantasy as a major motivating factor in human behavior: the external, historical event, and the traumatic response and its lifelong abreaction are the vital components of Binion's cause-and-effect psychological model. Binion's thesis also ignores the role of the autonomous ego beyond the grasp of instinct and the continuous interaction of psyche and society throughout life. He avoids the pitfalls of abstracting history out of life and onto the couch, but fails to appreciate fully that good (here, post-Freudian) theory illuminates. He prefers to make great leaps (early on he quite appropriately warns the reader to "hold tight") in tracing Hitler's words and actions back to his traumatic experiences. He unearths, for example, Hitler's many phobic references to cancer and thereby "proves" not only the existence of the specific trauma linked to his mother's illness but makes the reliving of it the determinative motivation in Hitler's political career. Hitler's fears (like Germany's), however, were *overdetermined*: they coursed developmentally from personal and cultural discontents, from a confluence of rational and irrational, and were projected onto the Jews as symbol, not as an equation between the Jewish Dr. Bloch and the Final Solution.

Ironically, in a book that is nothing if not documented, the weakest link in Binion's heavy evidentiary chain is his assertion that Hitler himself urged Bloch to use iodoform. (Indeed, Binion's whole argument involving the overdosage of iodoform has been disputed by Bloch's daughter.) Binion's source is Kubizek's recollection of Hitler's anguish over his mother's suffering: "Such a Hitler *would have urged* upon Bloch the drastic treatment that followed . . ." (p. 16, my italics). The hypothetical is true for Binion who deduces his case by "immersion" in Hitler's subsequent statements and acts. Such a methodology and conviction arise from Binion's faith in the value of empathy in psychohistory. He rather glibly dismisses both the difficulties inherent in the exclusive line of causation he adopts and the complexities of collective mental process (pp. xii-xiii). His very difficult style stems from the intimacy of his relationship with his subject: few specific analytical terms intrude into Hitler's world of "linking up," "covering," "throwing back," and "inform-

ing." One cannot slight Binion's attention to detail and his emphasis upon the irrational, but his method is suspect to the point of making this energetically intelligent contribution a necessarily controversial one.

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HANS STURMBERGER. *Adam Graf Herberstorff: Herrschaft und Freiheit im konfessionellen Zeitalter*. Munich: R. Oldenbourg Verlag. 1976. Pp. 518.

Hans Sturmberger, Director of the Upper Austrian *Landesarchiv* in Linz, has devoted much of his research to the introduction of Counter Reformation absolutism into the Habsburg lands and especially into Upper Austria. His biography of Georg Erasmus Tschernembl (1953) dealt with the long-time leader of the predominantly Protestant estates of Upper Austria, who led them to throw in their lot with the Bohemian rebels and so to provoke invasion of their land by Bavarian troops in the emperor's name in 1620. As Sturmberger points out, Herberstorff was not Tschernembl's counterpart; that was Emperor Ferdinand II, of whom Sturmberger has published a short, incisive study (1957). But as governor of Upper Austria from 1620 to 1628, when Maximilian of Bavaria held the territory as surety for the debts Ferdinand owed him, Herberstorff presided over the establishment of the new absolutism there. Like his earlier work, this book displays Sturmberger's unusual sensitivity to the intermixture of idealism and ferocity, religion and politics, that characterized the age. Well-documented and stylistically far superior to the study of Tschernembl, it is a model of historical biography and an important contribution to the understanding of the Counter Reformation in Central Europe.

Herberstorff has gone down in history as a tyrant. Sturmberger modifies this judgment and explains its origin not by rehabilitating Herberstorff—this he explicitly disavows—but by portraying him objectively against the background of his times. Herberstorff was born in Styria in 1585 of a Lutheran noble family. After about twelve years in the service of the Duke of Palatinate-Neuburg, during which time he converted to Catholicism, Herberstorff transferred in 1620 to Bavarian service, and Maximilian promptly named him governor of Upper Austria. Though generally competent and fair, his government was often rigorous, especially his treatment of the estates, who were unaccustomed to rule in the absolutist manner already common in Bavaria and elsewhere. By far the most serious discontent arose in 1624 following decrees imposing the Counter Reformation. Ferdinand, who retained sovereignty

over the territory, issued these at the urging of Herberstorff and despite the hesitation of Maximilian. But once Herberstorff encountered the resistance of the peasants to the removal of their pastors, he counseled a go-slow policy. Nevertheless, Ferdinand pressed on with more vigorous measures which were the chief cause of the Peasants' Revolt of 1626. Its subsequent suppression eliminated the last obstacles to Counter-Reformation absolutism. To a degree, Herberstorff became the scapegoat for the rebellion. Vienna was inclined to blame it on Bavarian rule, and the peasants held Herberstorff responsible for the Counter-Reformation measures of Vienna.

The experience of the occupation and rebellion created in Upper Austria a longing for return to Habsburg rule and a strong sense of regional identity. Upper Austria reverted to Habsburg control in 1628, but with the position of the estates greatly restricted. Partly to salvage Herberstorff's name, Ferdinand appointed him *Landeshauptmann*, that is, principal imperial representative in Upper Austria. Had death not taken him the next year, Herberstorff might have lived down his reputation, but he was to remain in Upper Austrian tradition a symbol of alien tyranny.

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RONALD G. WITT, *Coluccio Salutati and His Public Letters*. (Travaux d'Humanisme et Renaissance, number 151.) Geneva: Librairie Droz. 1976. Pp. xii, 112.

This study of Salutati's diplomatic correspondence was originally intended to be part of a larger work on the life and thought of this great fourteenth-century humanist and chancellor of Florence. The author made a wise choice in publishing this study separately. The subject has a significance which goes well beyond Salutati himself and raises important issues about the genesis and nature of humanism. Ronald G. Witt, after introductory chapters summarizing Salutati's career and describing the correspondence, addresses himself to these issues.

First, the diplomatic letter has long been considered an important form linking Renaissance humanism with its medieval roots. Paul Oscar Kristeller has suggested that the humanists were successors of the medieval *dictatores*, differing from these in using classical models for the style of the missives. Witt, in an excellent chapter analyzing the style of Salutati's public letters, shows that it remained much like that of his predecessors. The major influence of the classics lay less in style than in such subtle elements as an increased awareness of the power of oratory and of the importance of historical examples in argument.

Second, the diplomatic missives provide insights into the genesis of civic humanism. Hans Baron has argued that Salutati, after an early phase of republicanism, retreated to conventional political ideas in his old age. Witt's study generally supports this observation, though he stresses the vagueness of Salutati's concept of liberty, which was often used to mean simply good government under any particular form of polity. This vagueness, the author argues, makes it hard to be sure that his ideas were genuinely republican during the early part of his career.

Witt's book is a clear and useful study of an important topic.

DONALD J. WILCOX
University of New Hampshire

GIAN LUIGI BASINI, *Sul mercato di Modena tra cinque e seicento: Prezzi e salari*. (Università degli Studi di Parma Facoltà di Economia e Commercio, number 4.) Milan: A. Giuffrè Editore. 1974. Pp. xiv, 296. L. 5,300.

This book is little more than a compilation of prices at Modena from the mid-sixteenth century to 1700. Half of it consists of tables of prices of some fifty items (including wages and monetary values); the rest, after an introduction presenting the historical background and a discussion of the sources, is mostly a detailed description of the series, broken down into categories and periods. The data, as in most studies of this kind, come from accounts of religious organizations. The material is rich, and Gian Luigi Basini's handling of it is reasonably well informed by the vast literature of price studies for other places in Europe. The brief conclusion is mostly confined to the scene in Modena and does not reach a very high level of generalization about the overall performance of even that economy. Basini's data, however, confirm the general picture we have of prices in Italy: they began to rise in the late fifteenth century, became more inflated and subject to more erratic fluctuation in the sixteenth century, especially toward the end, and finally began to stabilize after the first quarter of the seventeenth century. We now have studies like Basini's for many places in Italy, and it is about time that someone subject them to sophisticated comparative analysis, so that we can bring all the data to bear on our larger view of Italy and at the same time get some better insights into the regional variations in its economic history during the early modern period.

RICHARD A. GOLDTHWAITE
Johns Hopkins University

SERGIO CAMERANI, editor. *Carteggi di Bettino Ricasoli*. Volume 26, 1 gennaio 1870–31 dicembre 1872. (Fonti

per la Storia d'Italia.) Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1974. Pp. 605. L. 10,000.

Of all the Risorgimento leaders Bettino Ricasoli was the least charismatic. Austere and aloof, he had difficulty mingling with his more loquacious and genial countrymen. He disdained the give and take of parliamentary life and the bluff heartiness of Victor Emmanuel, whose ancestry the Ricasolis matched century for century. Yet he played a crucial role in assuring the unity of Italy under the Savoy dynasty and in guiding the new state's policies after the death of Cavour. After 1867, when his second ministry fell from power, Ricasoli had no government responsibility other than that of deputy in Parliament, but the documentation in this volume clearly shows his continuing public importance. His opinion and counsel were sought by many, from Émile Ollivier, on a secret mission to seek Italian help against Prussia in August 1870, to Georg von Bunsen, who wrote from Berlin in July 1870, "rumor has it that there is a possibility you will return to power."

His Italian correspondents included cabinet ministers, politicians, journalists, businessmen, and reformers. They kept him informed about the latest government crises, traffic through the recently opened Suez Canal and its implications for Italy's future Mediterranean role, the candidacy of a Savoy prince to the throne of Spain, French problems after defeat, German ambitions. As a working member of Italy's entrepreneurial nobility, Ricasoli invested in many ventures: the importing of silkworms from Tashkent, railroad building in southern Italy, the planting of cedars of Lebanon in Italy, and the continued production of a prized Chianti on his family estate. Letters and reports from his agents and business associates demonstrate that Ricasoli was no absentee landlord or silent partner. Despite these wide-ranging interests, Ricasoli's overriding preoccupation continued to be the problem of Church reform. In July 1861 he had written to his daughter in words and tones worthy of a Luther that Rome was a "modern Babylon, more infamous than the ancient one, where the most nefarious deeds are committed against Italy and religion." Ten years later, in a twenty-page memorandum on the separation of Church and State, his language was more moderate but his views on the need for reform within the Church had changed little.

From the documents in this volume, drawn not only from the Ricasoli family papers at Brolio but from archives throughout Italy, much can be learned about Ricasoli the public servant, the religious reformer, and the businessman. What is missing is the private individual, for the "iron

baron" remains encased in his armor. But in documenting the concerns and the activities of Ricasoli the public figure, this volume offers important sources on Italian problems and crises in the early years of the state's existence.

EMILIANA P. NOETHER
University of Connecticut

BRUNO BONGIOVANNI and FABIO LEVI. *L'Università di Torino durante il fascismo: Le Facoltà umanistiche e il Politecnico*. (Collana dell'Istituto di Storia della Facoltà di Magistero dell'Università di Torino, number 9.) Turin: G. Giappichelli Editore. 1976. Pp. xxvi, 238. L. 4,000.

This book consists of two studies, one by Bruno Bongiovanni on the arts faculties at the University of Turin during the Fascist period, and the other by Fabio Levi on the Royal School of Engineering of Turin between 1923 and 1935. Bongiovanni's study is the broader of the two and deals with the role of the arts faculties in a city experiencing industrialization and in a milieu which was favorable to the expansion of technical education. He finds, however, that developments in this type of education were opposite to Gentile's intentions as outlined in the Reform of 1923. Rather than direct large numbers of students to the technical and professional schools, something which Gentile desired, the Reform discouraged the petty bourgeoisie from attending such schools and influenced them to select the culturally more prestigious schools leading to the university. According to Bongiovanni, the rejection of technical education and the subsequent expansion of the arts faculties bear witness to the capacity for passive resistance on the part of Turin's petty bourgeoisie against the Fascist regime and its efforts to impose its own schools and educational objectives.

In his study of the Royal School of Engineering of Turin between 1923 and 1935, Fabio Levi finds that at this school the Gentile Reform succeeded best and for a longer period of time in attaining its objective of reducing the imbalance between the supply and demand for graduates. He also finds that the school was relatively autonomous in relation to both the regime and industry, and that the typical engineer produced by the school was equipped with a balanced education which enabled him to adapt relatively easily to the world of industry.

Both studies are useful examinations of the social, political, and economic influences on two important sectors of education in Turin as well as of the way in which education provided an opportunity for passive anti-Fascist resistance and for access to the professions. Both authors have examined

considerable quantities of data on education policy, student population, administration, curriculum, ties with the Fascist regime, and relationships between education, state, and industry. They provide different but complementary contributions to our understanding of the social and cultural history of Italy under Fascism.

ELMIRO ARGENTO
Ottawa, Ontario

PAUL L. HORECKY and DAVID E. KRAUS, editors. *East Central and Southeast Europe: A Handbook of Library and Archival Resources in North America*. (Joint Committee on Eastern Europe Publication Series, number 3.) Santa Barbara, Calif.: Clio Press. 1976. Pp. xi, 466. \$35.75.

When the four European empires of the nineteenth century were superseded by a score of states after World War I, hardly a single library in the United States sought to arrange regular reception of publications from the new countries. Four decades later, the turmoil provoked by Sputnik spotlighted how woeful U.S. library holdings of materials relating to Eastern Europe had become. It also shook loose federal funds for use in setting up respectable collections.

This volume shows the stage reached by late 1972 in rectifying the shortcomings. Included are descriptions of the holdings of forty-three U.S. and Canadian libraries and archives concerning nine countries—Albania, Bulgaria, Czechoslovakia, East Germany (post-1949), Greece, Hungary, Poland, Romania, and Yugoslavia. The descriptions, arranged in alphabetical order by institution, were written by resident librarians or by outside specialists familiar with the holdings. Concentrating on the humanities and social sciences, they vary in length from seven lines to sixty-eight pages. Some information is provided on hours of operation, borrowing privileges, etc. The major libraries and archives are all included, although one could disagree on the selection of some of the lesser institutions. For instance, the Józef Piłsudski Institute is included, but the Orchard Lake (Michigan) Center for Polish Studies is not; the Jankola Library is included, but the Slovak Institute of Cleveland is not. The five years which elapsed from the completion of the individual surveys to publication is to be regretted, as are a number of presumably typographical errors.

As in most multi-authored volumes, the strength of the contributions varies. Some concentrate on individual volumes, while others are more concerned with periodical holdings. Some writers are more honest in describing what is lacking within a collection, but others ignore the lacunae. Too few mention receiving the many regional and univer-

sity publications begun in the last fifteen years, though these contain some of the most interesting recent research.

What we have then, despite the described shortcomings, is a useful, though expensive, survey which will permit researchers to more easily select where they can most profitably work. They would still be advised, however, to make specific inquiries before departing for any library or archive.

OWEN V. JOHNSON
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LESLIE C. TIHANY. *A History of Middle Europe: From the Earliest Times to the Age of the World Wars*. New Brunswick, N.J.: Rutgers University Press. 1976. Pp. xiv, 289. \$16.50.

Few tasks confound the historian quite as much as the writing of synthetic overviews covering a broad time span. Even bringing the best of recent secondary literature to bear raises exceedingly difficult methodological and organizational problems that tax the skills of the most gifted craftsmen. The task undertaken by Leslie C. Tihany in writing a history of the diverse lands of "Middle Europe" during the past millenium is an especially challenging, and indeed formidable, one. Regrettably, Tihany fails to meet the challenge successfully. This volume breaks no new ground either in its interpretation or in its use of sources. Restatements and summaries of the works of other scholars—perhaps inevitable in this type of study—are brought together in a somewhat confused and unsystematic manner.

In his introduction the author insists that: "To be progressive as well as cumulative the presentation must include the most recent research published in nonwestern languages." However, evidence that such sources have been consulted is conspicuously absent but for occasional references to recent Hungarian and Soviet literature. Indeed, a major problem with this study is the author's apparent unfamiliarity with basic Western language sources such as L. S. Stavrianos' *The Balkans Since 1453*, M. S. Anderson's *The Eastern Question*, William H. McNeill's *Europe's Steppe Frontier*, and Alexander Gieysztor's *History of Poland*. These deficiencies are epitomized by his assertion that R. J. Kerner's *Slavic Europe: A Selected Bibliography in Western Languages* remains "the standard bibliographic guide" for Slavic Europe. Overlooked are Paul Horecky's 1969 publications *Southeastern Europe: A Guide to Basic Publications* and *East-Central Europe: A Guide to Basic Publications*.

The shallowness of the research no doubt accounts in large measure for the plethora of disquieting errors, oversimplifications, and omis-

sions. For example, "Illyrianism" is characterized as "the early eighteenth century protonationalist movement of the South Slavs emerging from Ottoman domination"; the 1809 Treaty of Schönbrunn is renamed the Treaty of Vienna; the Serbs are reported to have held a series of "national congresses" in the mid-eighteenth century; the analysis of the peculiar differences between "landlocked Europe" and "coastal Europe" categorizes continental-minded France as a coastal region; reference is made to an "emerging Ottoman Foreign Office" at the dawn of the eighteenth century; Tsar Nicholas I of Russia is described as "an antinationalist at home" and "a firm protagonist of Greek independence at the expense of the Ottoman Empire"; and a brief chapter is included on the advent of socialism but the revolutionary impact of capitalism upon the tradition-bound societies of "Middle Europe" is completely ignored.

The book's shortcomings make it unsuitable for the scholar in addition to limiting severely its usefulness among non-specialists interested in the historical development of Middle Europe. One wonders why the various sins of omission and commission were not expurgated during the publication process.

LAWRENCE P. MERIAGE
University of Kansas

MICHAEL BORO PETROVICH. *A History of Modern Serbia, 1804-1918*. In two volumes. New York: Harcourt Brace Jovanovich. 1976. Pp. xx, 359; 361-731. \$50.00.

The Serbian people have had a checkered history of wars, occupation, cultural and artistic achievement, national tragedy, and melodrama as rich and varied as that of any other ethnic group in Europe. Their early and medieval history was excellently chronicled by the nineteenth-century Czech historian Jiriček, and more recently by D. Obolenski. Until now there has been no major history of modern Serbia in the English language, as the sympathetic history written by H. Temperley during the First World War was of necessity limited by its provenance. Thus Michael Boro Petrovich's two-volume *History of Modern Serbia* excellently fulfils a long-standing need. Although written with the warm understanding and commitment of his own personal roots in the region, it is also a factual account by an expert historian with as much objectivity and perspective as scholarship can produce. Petrovich manages to make a highly complex and involved subject appear deceptively easy, not by oversimplification but by the depth of his own understanding and the clarity of his exposition.

The basic arrangement is chronological, with volume one covering the period of the evolution of the Serbian state to 1868, and volume two dealing with the period from the accession of Milan Obrenović through the First World War until Serbia was merged into the Kingdom of Serbs, Croats, and Slovenes in 1918. It is the history of what today would be called a developing country, and Petrovich deals with this history of nineteenth-century Serbia in an appropriately modern way. While explaining complicated and often devious political developments, he shows how closely they were interwoven with internal and external economic affairs. He also manages to include social history and interesting accounts of most of the outstanding Serbian writers and other public figures—and for Petrovich this naturally includes Montenegrins though that country was a separate principality/kingdom until the foundation of Yugoslavia. Although he gives full attention to external and some internal developments in Serbia during the reign of King Peter I in the twentieth century, it is a pity that he relates very little (and nothing new) about the centers and exercise of power in Serbia between the murder of King Milan Obrenović in 1903 and King Peter Karadjordjević's own abdication in favor of his second son, who became King Alexander I of Yugoslavia. These two volumes have good maps and interesting illustrations but there are no footnotes. The guide for further reading, though helpful, does not serve as a substitute for those interested in sources. These are minor criticisms of a work that is informative, easy to read, and unique in its field.

PHYLLIS AUTY
Simon Fraser University

DŽEVAD JUZBAŠIĆ. *Izgradnja željeznica u Bosni i Hercegovini u svjetlu austrougarske politike od okupacije do kraja Kállayeve ere* [The Building of Railways in Bosnia and Hercegovina in Light of Austro-Hungarian Policy from the Occupation to the End of the Kállay Era]. (Odjeljenje društvenih nauka, number 28.) Sarajevo: Akademija Nauka i Umjetnosti Bosne i Hercegovine. 1974. Pp. 285. ND 85.

From the beginning of Austria-Hungary's occupation of Bosnia and Hercegovina in 1878, political leaders in the Dual Monarchy sharply disagreed on the extent and purpose of railway construction in the two provinces. Dževad Juzbašić, a scholar at the Institute for History in Sarajevo, has produced a richly documented study of the complex struggles within the Monarchy which preceded decisions to build key railway lines. If anything, his work bears too modest a title. Rather than merely describing railway construction from 1878 to 1903, Juzbašić ably illuminates the efforts of

various interest groups to influence the Monarchy's economic policies in its Balkan colony.

Foremost among the advocates were the economic elites in the Austrian and Hungarian halves of the Monarchy, acting through the media of their respective governments. Juzbašić contends that the Hungarian government was especially unscrupulous in promoting private interests at the expense of a far-sighted development plan for Bosnia and Hercegovina, yet the reader is struck by the consistency with which both governments blatantly acted as surrogates for leading economic circles within their constituencies.

The Austrian government, with a well developed economic base, favored lines connecting Vienna with Sarajevo to facilitate the importation of Bosnian raw materials and the sale of Austrian manufactured goods in the Bosnian market area. The Hungarians, jealous of Austrian industrial superiority, advocated lines which would give their manufactured products a competitive edge in the newly acquired provinces. The two governments came into increasing conflict in the 1890s after both had experienced a change in leadership.

Benjamin von Kállay, joint finance minister from 1882 to 1903, wished above all to promote rapid economic growth in the provinces. However, he made key concessions to military leaders who favored building railways to promote the Monarchy's strategic expansion into the Balkans. The bitter conflicts between intra-Monarchy factions stymied the implementation of a rational, consistent plan for railway construction.

Juzbašić closely follows the sources in his account, but at some points he is limited in vision by his devotion to detail. The book needs either more maps or fewer references to obscure towns. The author has, nonetheless, performed a valuable service by bringing together an impressive array of documentation from many archives. The end product not only adds to our understanding of Bosnian history, it also offers many insights into the labyrinthine decision-making processes of Habsburg Dualism.

ROBERT DONIA
University of Michigan

HUGH SETON-WATSON *et al.*, editors. *R. W. Seton-Watson and the Yugoslavs: Correspondence, 1906-1941*. In two volumes. Zagreb: University of Zagreb, Institute of Croatian History. 1976. Pp. 474; 468.

Few non-Yugoslavs have moved with as much ease or as great an influence through the pages of twentieth-century Yugoslav history as R. W. Seton-Watson. From 1906 until the coming of Hitler's armies in 1941, Seton-Watson took an active interest in Yugoslav affairs, writing for audiences

in Britain and Yugoslavia and speaking personally to and on behalf of the Yugoslavs he knew so well. His efforts did not always succeed, but he stands nonetheless as one of the most vital spokesmen of the Yugoslav cause in this century.

Scholars now have available a collection of Seton-Watson's letters, articles, and notes of conversations. Edited jointly by Hugh and Christopher Seton-Watson (R. W.'s son and grandson, and each a distinguished historian in his own right), and by leading scholars of the Institute of Croatian History, who have over the past decade produced a series of outstanding monographs on interwar Yugoslavia, this two-volume set, published in both an English and a Serbo-Croatian edition, is an excellent collection. Seton-Watson's circle of friends was large—as the collection demonstrates—and included such important figures as Heinrich Friedjung, Svetozar Pribičević, Frano Supilo, Ante Trumbić, Stjepan Radić, and Jovan Jovanović. With the exception of Agrarian Party chief Jovanović, Seton-Watson's principal correspondents were political figures from the old Habsburg section of Yugoslavia. The skewed nature of his contact clearly influenced his judgment, and, while he mistrusted the extremist views of some of his Croatian friends, he blamed the Serbian politicians, especially Pašić and the Radical Party, for many of Yugoslavia's interwar difficulties. That bias notwithstanding, Seton-Watson's personal insights, as well as his reporting of the views of almost all the major Yugoslav factions before, during, and after World War I, are an invaluable resource. The letters are carefully identified by date and place of origin and in their original language, principally English, French, and German, with fewer examples in Serbo-Croatian and Italian.

In a fine introductory essay, Hugh and Christopher Seton-Watson portray R. W. as a man of moderation and reason, disappointed at times by what he perceived as the shortsightedness or selfish ambitions of British and Yugoslav politicians, but perpetually hopeful that more perceptive leaders would eventually emerge. His optimism stemmed, as the authors note, from his own experience as an ardent Scotsman living in harmony with the English under a single flag. They trace eloquently the development of his interest in and awareness of the Yugoslav situation and explain his periodic changes of heart, such as his turn from a support of trialism within the Habsburg Empire to his advocacy of Yugoslav separatism. They do not elucidate, however, his reasons for shifting from the position of the Yugoslav Committee in 1918 to support at Versailles the program of the Serbian-dominated government. Seton-Watson recognized what he believed to be the legitimate rights of national minorities, and he spoke and

wrote vigorously on their behalf. But he never realized that the same national feelings which resisted oppression in some circumstances might cause it in others. He ignored the ample evidence, some of it expressed by his Yugoslav correspondents, of the darker side of nationalism, and his heirs fail to challenge him in this respect. Their essay, a prelude to their forthcoming book on the same subject, is nevertheless a fine introduction to these volumes.

Appended to the collection is a highly useful set of annotated and cross-referenced footnotes, marred only by two missing notes (Vol. I, Doc. 169; Vol. II, Doc. 140), and an even better Register of Documents, giving pertinent information about each document with a synopsis of its contents. Together with the concluding catalogue of correspondents, identified in the index rather than with this list where it would have been more immediately useful, the register provides an ideal reference. All in all, this collection is one of the best contributions to emerge from Yugoslavia in recent decades. Invaluable to students of Yugoslav affairs, it is also useful to scholars of British history or those studying more particular historical issues, such as the origins of World War I, the disintegration of the Austrian Empire, or the breakdown of democracy in Eastern Europe between the wars. With the few exceptions cited above, it is exquisitely edited and deserves to be viewed as a model by editors of similar volumes in the future.

BRUCE E. BIGELOW
Denison University

RASTISLAV TERZIOSKI. *Denatsionalizatorskata dejnost na bugarskite kulturno-prosvetni institutsii vo Makedonija (Skopska i Bitolska okupatsiona oblast), 1941-1944* [The Denationalizing Activity of Bulgarian Cultural-Educational Institutions of Macedonia (Skopje and Bitola Occupation District), 1941-1944]. Skopje: Institut za natsionalna istorija. 1974. Pp. 424.

GLIGOR TODOROVSKI. *Okupatsijata na Zapadna Makedonija* [The Occupation of Western Macedonia]. Skopje: Misl. 1975. Pp. 200. ND 60.

In recent years the number of studies dealing with conditions in Southeastern Europe during World War II has increased considerably. The books under review deal with the ideological, cultural, educational, and social policies pursued during the war years by the governments of Bulgaria and Albania and the local collaborationist authorities in Yugoslav Macedonia. The works treat also the reaction and struggle of the local population against the policies of the two occupying states as well as the Italian and German fascists.

Rastislav Terzioski's thesis is that the forms and means of the Bulgarian government's educational and cultural policies were aimed particularly at the Bulgarianization of the youth, the assimilation and denationalization of the Macedonian people, and the total incorporation of Macedonia into the Bulgarian state. These, the author states, were the traditional goals of the Bulgarian bourgeoisie which during the war years was imbued with fascist ideas. One of the Bulgarian government's means to these ends was through control of the schools, communication networks, and entertainment. The Bulgarian authorities not only introduced the Bulgarian educational system in Macedonia but expanded it; the Bulgarian plans called for making education available to all the people the authorities considered Bulgarian. Primary schools were founded in villages, secondary schools in towns, and a university in Skopje. Terzioski regards the Bulgarian educational and cultural effort in Macedonia as nothing more than a form of ideological aggression aimed at the denationalization of the Macedonian people. There is a whole chapter dealing with the policies the authorities enacted in treating such problems as teaching personnel, subjects taught, and relations between schools in Bulgaria and Macedonia. The Bulgarians, he states, created special Bulgarian language courses because they realized that the Macedonians were not Bulgarians and the language imposed by the authorities was foreign to them. Another chapter examines the organization of special classes for the moral upbringing of students and the use by the authorities of a strict discipline code of behavior aimed at denationalizing the Macedonian youth and using it in the struggle against Communism.

An entire chapter treats the aims and activities of *Branik*, *Otets Paisii*, and other pro-fascist youth organizations and the attempts made by the authorities to induce the Macedonians to join these organizations and fight the anti-fascist movement in Macedonia. In about fifty pages the author examines the denationalizing activity of the Bulgarian Department for National Propaganda in Macedonia. Through control of all public and private cultural institutions, clubs, professional associations, publishing houses, libraries, museums, theaters, radio broadcasting, and in collaboration with the Bulgarian Orthodox Church, this department aimed at negating "the existence of Macedonian national individuality" and in convincing the Macedonians that they were Bulgarians. The final chapter traces the development of the resistance movement of the Macedonians against the denationalizing activity of the Bulgarian cultural-educational institutions. The author states that

under the leadership of the Yugoslav Communist Party and the Union of Communist Youth the Macedonian people and especially the Macedonian students were able to frustrate the plans of the Bulgarian fascists.

Gligor Todorovski regards his work as an introduction to the study of the Albanian occupation of Western Macedonia. After a brief account of the struggle for the division of Macedonia into zones of occupation and influence, he traces the possession of Western Macedonia by Italy, its annexation by the Albanian government, and the coming of the special German military forces of occupation. Six chapters are devoted to the Italo-Bulgarian discussions relating to Western Macedonia. The author shows that although Italy and Bulgaria belonged to the Tripartite Pact and had concluded a number of friendly agreements, relations between the two countries were never friendly: Bulgaria never gave up the idea that Western Macedonia was its territory, Italy identified its interests with those of Albania, its colony.

Todorovski's chapters on the economic and social policies of the Albanian authorities in Western Macedonia are of considerable interest. As the Albanians occupied Western Macedonia they reintroduced a number of old institutions dating back to the Ottoman period. The tithe was introduced and former collectors demanded that they have their old jobs or be paid for their losses. The so-called *seimenlak* was also re-established. Former policemen or village guards and/or the descendants of such, were given full powers to rule over villages and whole regions. Todorovski, like Terzioski, writes about the policies of denationalization pursued by the Albanian authorities as well as the attempts to artificially create enmity between Macedonians and Albanians. Todorovski also deals with the resistance of the Macedonian masses and the collaboration between Macedonians and Albanians in the struggle against the Italians, Germans, and collaborationist Albanian forces.

At the end of Todorovski's work is an appendix of documents he used. Terzioski's volume has a bibliography and an index. Both authors have used non-published materials in their studies, but the full story of conditions in Macedonia cannot be written until all archival materials are made available to all scholars.

The analysis of the Bulgarian institutions in Macedonia would have been enhanced by a brief comparison with prewar Yugoslav institutions. In light of the author's argument about opposition to Bulgarization some attention should have been given to the stand of the Bulgarian Communist Party which was itself opposed to the Bulgarian

occupation. Although these books provide much new information, the authors fail to substantiate their tacit assumption that the entire Slav population had Macedonian national consciousness.

PHILIP SHASHKO
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ZINA MARKOVA. *B'lgarskoto ts'rkovno-natsionalno dvizhenie do Krimskata voina* [The Bulgarian National Ecclesiastical Movement before the Crimean War]. (Izsledvaniia po b'lgarska istoriia, number 1.) Summary in French. Sofia: Izdatelstvo na B'lgarskata Akademiia na Naukite. 1976. Pp. 220. 2.34 lv.

For several years Bulgarian historians have been devoting the bulk of their efforts to the preparation of a multivolume *History of Bulgaria* to celebrate their centennial of modern independence and the thirteen-hundredth anniversary of the founding of the first Bulgarian state. As a by-product of this work the Historical Institute of the Bulgarian Academy of Sciences is proposing to inaugurate a new series, "Studies in Bulgarian History," to publish documents and monographs on historical problems that so far have been "inadequately treated and discussed." Zina Markova's study of the origins of the movement for Bulgarian ecclesiastical independence is the first volume in the series. If future ones are of equal quality it will become a valuable addition to Bulgarian historiography.

Markova begins her work by surveying the history of the Church in Bulgaria from the time of the Ottoman conquest. Rejecting the outmoded notion of the "Greek cultural yoke," she credits the Ecumenical Patriarchate with playing a significant part in preserving both religious and secular culture from Turkish influence. She links the rise of the movement for ecclesiastical independence at the beginning of the nineteenth century to the growing nationalism of the Greek clergy, the emergence of Bulgarian artisans and traders seeking to challenge the clergy for control of local affairs, and the current of reform within the Ottoman Empire, particularly the promulgation of the *Hatti Sherif*, which provided a legal basis for Bulgarian protests.

The author deals extensively with the petitions to the Ottoman government of Neofit Bozveli and Ilarion Makariopolski, which, along with the official recognition extended to the Bulgarian community in Constantinople, she regards as watersheds in the movement. Her final chapter on the Church question in its international context concludes that the indifference of the Western Powers and Russia's commitment to maintaining the in-

tegrity of the Ecumenical Patriarchate left the Bulgarians no alternative to looking to the Turks for support.

Markova's efforts to demonstrate that the movement for ecclesiastical independence, despite its loyalist tactics, was "objectively revolutionary," will strike some as labored. Nevertheless, it should not detract from her achievement of casting a great deal of light on a little-known chapter of Bulgarian history.

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ILCHO DIMITROV. *B'lgaro-italianski politicheski otnosheniia, 1922/1943* [Bulgaro-Italian Political Relations, 1922-43]. Summary in English. Sofia: Izdatelstvo nauka i izkustvo. 1976. Pp. 471. 3.79 lv.

Ilcho Dimitrov, who has already made important contributions to the political history of Bulgaria between the two world wars, now offers a detailed study of the political interconnections between Mussolini's Italy and a Bulgarian state which experienced many upheavals between 1922 and 1943. The book relies upon thorough research in Italian and Bulgarian archives and presents its findings in relatively objective fashion (in today's Bulgaria, of course, there are some risks in choosing to deal with such a topic at all). Neither the detail nor the overall conclusions—which seem quite acceptable, though couched in Marxist terminology—are very startling, but the book is quite helpful. Its chief shortcoming is a function of its virtues: it is too detailed to be easily read as a consecutive narrative, and so comes to resemble a reference book.

The focus of Dimitrov's attention varies historically with the internal Bulgarian political situation. At times when political parties were either forbidden or severely restricted—during the several years after Alexander Tsankov's 1923 coup against Alexander Stamboliyski, or the years after 1935, when King Boris concentrated all power in his hands during the "personal regime"—Italian attention was directed toward government officials. Consequently, for these periods the book becomes a history of diplomacy and foreign relations, a history which depends heavily upon particular personalities and was no doubt fascinating for contemporaries but which for readers of a few decades later is a dull affair. On the other hand, from the late 1920s until the military coup of 1934, political organizations functioned rather freely within Bulgaria, and thus Dimitrov can produce something resembling more an internal political history with special reference to Italian interests. Here he demonstrates that Italian diplomats courted a few very minor domestic political groupings

which looked to Italy for ideological inspiration. Their influence was minuscule, though, and the only important pro-Fascist effort of the 1930s, the Tsankov movement, took its lead from Germany. Here, as so often, Italy found itself relegated to the Bulgarian sidelines.

Indeed the entire history of Italian policy in Bulgaria over this period is one of nearly unrelieved frustration. True, King Boris did marry into Italian royalty and even journeyed to Italy for the ceremony, but he always realized that Italy was not a prime factor in contemporary international relations. Except for a brief time at the inception of the Tsankov regime, Dimitrov concludes, Italy exercised little influence in Bulgaria during the 1920s; and when Bulgarian soil proved more receptive to Fascist influence, it was the German variety which attracted the admiration of Bulgarian political leaders both inside and outside the government, including King Boris, who essentially controlled the country from about 1936 until his death in 1943. Dimitrov's current book thus describes carefully and in detail a relationship of only secondary importance for interwar Bulgaria.

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DANIEL CHIROT. *Social Change in a Peripheral Society: The Creation of a Balkan Colony*. New York: Academic Press. 1976. Pp. xvii, 179. \$12.50.

This essay seeks to explain the dynamics of social development in Wallachia from the thirteenth to the twentieth centuries and is a welcome contribution to Romanian historiography. Rather than pursue the course of most scholars in this field and carve out a brief period to study in overwhelming detail, Daniel Chirot aims to describe the social relations and the economic factors which shaped the history of lord and peasant in Wallachia for nearly the entire history of the principality. He answers the question of how Wallachia came to be a "backward economic area with a peasant problem."

He divides Wallachian history into three categories based on the dominant political economy of the era. In his view, the principality was originally a communal trading society, a major point in the Levantine trade network, with a bureaucracy deriving its revenue from taxes on commerce and a free communal village structure based on animal husbandry. This system lasted from the formation of the state until the sixteenth century when the Ottoman conquests and Portuguese discoveries of new routes to the East altered trade patterns. Wallachia then became a proto-colonial society subject to the Turks, and a new source of economic wealth

had to be found: agricultural surplus. This new situation created tension between the *boiers* or nobles and the peasants which was to last until well into the present century. With the decline of Ottoman power over Wallachia, Chirot sees the transformation of the principality into a colony of the Western capitalist market, its income based on cereal exports to Western Europe.

In other words, the author attempts to relate Wallachian history in terms of contemporary categories of social theory. He bases his framework on a wide variety of theoretical studies but he also has a firm grasp of the best recent and traditional scholarship on Romania. Novel as his approach may seem for Romanian history, his views generally fell within the perimeters of the major schools of interpretation while offering a new slant on them.

One of my few reservations centers on the Peasant Uprising of 1907. Following Eric Hobsbawm, Chirot views the movement as a violent reaction to the expansion of the capitalist market into less developed rural areas. Peasant revolts, however, have a long history in the Romanian lands and in Eastern Europe generally. In what way does the violence of 1907 differ from previous uprisings, apart from its scale? I wish the author had dealt more fully with this problem, though the conception may have precluded such discussion.

This essay points to the direction in which more scholarship in the field should move. We need more broad analytical studies and perhaps fewer exhaustive depictions of agrarian conditions in a given county for a given decade. Students of Balkan history and of social history in general should find Chirot's book quite stimulating.

LEONARD OLSON
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GÉZA KATHONA. *Fejezetek a török hódoltsági reformáció történetéből* [Chapters from the History of the Hungarian Reformation during the Turkish Occupation]. (Humanizmus és Reformáció, number 4.) Budapest: Akadémiai Kiadó. 1974. Pp. 251. 55 Ft.

This is a history of the rise and decline of the school of the market-town of Tolna, located in southwestern Hungary, during the second half of the sixteenth century. Thanks to its strategic location, the town survived the Turkish conquest by five decades as a lively commercial and intellectual center.

The rise of the Tolna school was intimately associated with the spread of Calvinism in Hungary. It not only provided an excellent education to Hungarians cut off from the West, but it also raised a generation of Calvinist ministers and a lay in-

telligentsia recruited largely from children of local merchants and burghers.

The school maintained close relations with Calvinist centers of learning in Hungary and abroad. It sent its matriculated students to Kolozsvár and its teachers visited Debrecen and Sárospatak. Many of its students went abroad to continue their education at Western universities. Unfortunately, the school declined by the end of the century, mainly as a result of the Fifteen Years' War. By 1602, most of its activities were transferred to Kecs-kémét.

Besides the historical narrative, this volume contains several significant documents. Two of these, the letter of Pál Thúri Farkas to his friends in Upper Hungary and a brief contemporary account of the life of István Szegedi Kis, will be important for social historians. The book also contains a list of the teachers of Tolna between 1549-1602 and their contributions to Hungarian literature.

One often wishes, however, that the author had spent more time in discussing the influence of the Tolna school in the region; this influence was felt as far south as Croatia and Serbia, and would provide important new insights for historians of the Reformation. Valuable as this volume is, it only underscores the need for a good history of Hungarian Protestantism that has not yet been written in any language.

JOSEPH HELD
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GYULA BORBÁNDI. *Der Ungarische Populismus*. (Studia Hungarica, number 7.) Munich: Ungarisches Institut München. 1976. Pp. 358.

Gyula Borbándi calls this work a "modest attempt," yet it is the first comprehensive history and—probably for many years to come—the definitive history of Hungarian populism, a unique literary and political movement of the interwar years with repercussions which have lasted to our day. Hungarian populism had its parallels abroad, but while the much older Russian *narodnichestvo* tended to be terroristic and conspiratorial, the German *völkische Bewegung* reactionary and racist, U.S. populism purely political, and Romanian "poporanism" purely literary, the Hungarian movement was strictly legal, patriotic rather than chauvinistic, progressive rather than reactionary, and, most important, influential in both literature and politics. Mostly of peasant origin themselves, the populists first set out to discover the peasantry, a great unknown in agrarian and semifederal Hungary. The picture they drew of rural life was that of ignorance and misery but also of inestimable traditional values which, these "village explorers" in-

sisted, should not only be safeguarded but used to rejuvenate Hungarian society. Peasant conditions were to be improved and Hungary as a whole geared to the needs of peasants. Small wonder then that contemporaries saw populism as either hopelessly romantic or as eminently realistic, and that the populists were both courted and abominated by Admiral Horthy's conservative system, the middle class, the underground Communists, and the Arrow-Cross national socialists. And small wonder that the populists themselves were profoundly divided about their politics. Only a few "westernizers" or "urbanists" perceived populism as unreservedly destructive, a symptom of decay rather than a panacea for backward, oriental, isolated Hungary. The debate is not dead. Hungarian Communism today harbors a strong populist trend—socially egalitarian and politically orthodox—as opposed to an urbanist trend which is economically and politically more liberal. Behind it all lurks the age-old antagonism between those tied to the land and the largely alien city population, and between Christians and Jews.

Borbándi, himself a former populist and now a well-known literary critic and coeditor of the Munich-based *Uj Látóhatár* (New Horizon)—the Hungarian emigration's best literary/political journal—is, of course, sympathetic to populism, but his scholarship is irreproachable. The first three chapters of his book are a fine survey of Hungarian history—cultural, economic, and political—between 1919 and 1949; the next five chapters deal with populism until 1945, and the last three chapters with populism since World War II. For Borbándi, the history of the movement was marked not only by many ups and downs, but by continuous political vivacity. For this reviewer, it was marked by great literary accomplishments and bad politics. The village explorers wrote magnificent sociography, but they could not translate their findings into a cohesive political program. Some advocated a "Third Road"—neither socialism nor capitalism—but none could explain what that meant. During World War II there were a few brave anti-German initiatives, but most populists failed to oppose Fascism and violent anti-Semitism openly. After World War II, most populists failed to oppose the Communists. There were heroic exceptions of course, but around 1948, when the Stalinist tyrant Mátyás Rákosi destroyed the populist "people's colleges" and the populists' own Peasant Party, both institutions deserved their fate. The party made a brief comeback in the Revolution of 1956, but now it belongs to history. Populism was fascinating, and among its many nonfictional products this is one of the best. Borbándi's style is solid and, despite a slight penchant for lexicographic approach, the book makes absorbing read-

ing. The brief biographies and the bibliography are superb.

ISTVÁN DEÁK
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PETER PASTOR. *Hungary between Wilson and Lenin: The Hungarian Revolution of 1918-1919 and the Big Three*. (East European Monographs, number 20.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1976. Pp. vi, 191. \$13.00.

Peter Pastor set himself a difficult task: "to fill the gap which was probably caused by Professor [Arno J.] Mayer's lack of Magyar and by the [un]availability of archival documents, still closed at the time of the publication of his award-winning book," *Politics and Diplomacy of Peacemaking* (p. iv). Whatever we may think of Mayer's interpretation of the anti-Communist stance of the peacemakers, we must grant that his scholarship is meticulous, his prose highly readable, and his world-view erudite. The same cannot be said of *Hungary between Wilson and Lenin*.

The book's main thesis is an extension of Mayer's contention that "Count Michael Károlyi invited the Bolsheviks into the Hungarian government in order to make his threats [of Bolshevism] more credible" (p. 10). But while the author of *Politics and Diplomacy of Peacemaking* was content to give his revisionist imprimatur to the old conservative and right radical accusations about Károlyi's motives, Pastor goes a step farther. He portrays Károlyi as a revolutionary who, months before the establishment of the Hungarian Soviet Republic on March 21, 1919, contemplated a Soviet-Hungarian alliance against the Western imperialist powers. In support of this thesis, he offers a questionable interpretation of an unreliable report of what Oszkár Jászi said to a group of Romanians in November 1918; a remark by Zsigmond Kunfi, a left-wing Social Democrat in Károlyi's ministry; a passage quoted out of context from a memorandum by Károlyi concerning Hungarian relations with Russia, and a diary entry by Countess Károlyi which does not even mention Soviet Russia.

According to Pastor, Károlyi turned toward Russia in response to Anglo-American aloofness and French hostility. The French were the most vehement anti-Bolsheviks; in order to contain Hungarian Communism, he asserts, they favored the use of occupation troops ("and, in fact, intervention to this end was already under way in Hungary") and a reduction of Hungary's size (p. 96). But "the fear of Bolshevism forced the hands of even those Allied policy-makers who seemed determined to remain objective and impartial" (p. 149). It was this very Allied fear of Bolshevism, Pastor

claims, which drove Hungary to Bolshevism. He does not, however, provide sufficient documentary evidence to convince the reader of such a grand irony of history.

The research for this volume was extensive; Pastor consulted manuscript collections in American, British, and French archives and read a wide range of published sources. Unfortunately, the final text falls far short of the impressiveness of his research. A forced thesis is not the only problem; the book contains numerous factual errors, misidentifications, and inaccurate footnotes. As a result, it can only confuse the uninitiated and disappoint the experts.

EVA S. BALOGH
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DÁNIEL CSATÁRI. *Dans la tourmente: Les relations hongaro-roumaines de 1940 à 1945*. Translated by AIMÉE MARTEL. Budapest: Akadémiai Kiadó. 1974. Pp. 419. \$20.00.

This substantial study was completed in its original form in 1961 and presented as a doctoral thesis in 1963. It examines Hungarian-Romanian relations in the tragic years of the Second World War, when Nazi Germany and the Soviet Union used the conflicting national aspirations of Hungary and Romania for their own purposes. Basically the narrative is a Marxist interpretation of history, strengthened with the wisdom of hindsight, and based on a large number of documents made available to the author in the Hungarian Foreign Ministry and the office of the Hungarian prime minister. The Romanian documentary sources are less numerous. Besides diplomatic developments, the volume contains detailed description of abuses and atrocities committed in both countries against national minorities, Communists, and persons of left-wing views.

Events are interpreted according to the official version of history in the Soviet-dominated peoples' democracies and there are conspicuous omissions and inaccuracies. The author correctly points out that both the Hungarian and Romanian governments initiated armistice negotiations with Britain and the United States. He characterizes these moves as maneuvers of doomed regimes, but fails to mention that both in Hungary and Romania the overwhelming majority of the population, symbolized by the democratic opposition parties, expected an Anglo-American occupation of the Danubian area and made preparations for a democratic transformation of domestic political life. Dániel Csátári relates with gusto how King Michael dismissed and arrested the pro-Nazi dictator, General Ion Antonescu, and his prime minister, Mihai Antonescu. He fails to mention that

by the end of February 1945 Andrei Vyshinsky, Soviet Deputy Commissar of Foreign Affairs, traveled to Bucharest and in a stormy session demanded that Michael dismiss Radescu's coalition government and appoint Petru Groza as premier. Groza was eager to form a Communist-dominated government. When the King at first refused to accept the Soviet ultimatum, Vyshinsky threatened that Romania might cease to exist as a sovereign state. Csátári does praise the Groza government for punishing those Romanians who mistreated Hungarians in Transylvania and for proclaiming an egalitarian policy toward the minorities. The latter policy was understandable: it was in the interest of the Soviet Union and Romania that, during the period preceding the conclusion of peace, relations between Hungarians and Romanians improve in Transylvania and demonstrate the promise of satisfactory developments. As is well known, this policy later changed drastically.

STEPHEN D. KERTESZ
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PAUL DE VOOGHT. *L'hérésie de Jean Huss*. In two volumes. (Bibliothèque de la Revue d'Histoire Ecclésiastique, numbers 34 and 35.) 2d rev. ed. Louvain: Publications Universitaires de Louvain. 1975. Pp. xxii, 517; 322-1032.

The two volumes presented here under the same title are new editions of books first published in 1960, the first bearing the present title, the second—a collection of articles and texts—entitled *Hussiana*. The first volume, a study of the Bohemian religious movement, John Hus' relation to it, Hus' doctrine, and his condemnation by the Council of Constance, attracted much attention when it appeared because of its technical mastery (Paul De Vooght is perfectly abreast of the Czech literature for one thing, and his familiarity with Hus' work seems impeccable) combined with its liberal-Catholic viewpoint. No other Catholic scholar had presented a Hus so remote from the vicious heretic condemned by the Church, a reformer so authentically Christian and indeed Catholic except for a few errors in doctrine and some indiscretions of language. Yet De Vooght had to conclude, "I admit that Hus was a heretic," because he had denied the divine institution of the papacy. Fifteen years later De Vooght has not changed his mind (p. 501), but has changed the sentence in question to "I admit that Hus undoubtedly professed a heresy," and has added a fervent hope that "the day may come when Rome will quash the unjust and baleful verdict pronounced against Hus by the Council of Constance" (p. 517).

For the rest, the new edition of this volume has been brought up to date in its annotations and sometimes in the text; no revision has been thought necessary in the argument or general picture. While Hus was, in my opinion, more subversive than De Vooght allows, more closely linked in spirit and in fact to some of his indubitably heretical associates and followers (from whom De Vooght tries to pry him loose), there can be no argument with the analysis of Hus' writings offered here and the work can be praised once again without further reservation.

The second volume, of special studies, has also been brought up to date in its notes and a few new essays have been added, chiefly one on Hus as a philosopher, another attacking various Marxist interpretations of Hus as a revolutionary, and a third defending the author's refusal to moderate his debunking of the legend of St. John of Nepomuk. The original essays retain their great value; the most important perhaps are "Catholic Ecclesiology in Prague around 1400," "*Universitas praedestinatorum* and *Congregatio fidelium* in John Hus' Ecclesiology," and "The 'Simoniacal Heresy' from Saint Thomas Aquinas to John Hus." Two substantial Latin texts are also presented: John of Jenštejn's treatise on the Church and his *Acta in curia Romana*; there are also three short texts dealing with indulgences. It is an exceptionally important volume for specialists; those interested more generally in John Hus will not need volume two, but if they do not buy it they will miss the index to the whole.

HOWARD KAMINSKY
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PETER BROCK. *The Slovak National Awakening: An Essay in the Intellectual History of East Central Europe*. Buffalo: University of Toronto Press. 1976. Pp. x, 104. \$12.50.

In old Hungary the political *natio hungarica* included members of different ethnic groups. The peasantry spoke a variety of local dialects, and the educated classes read and wrote in more than one language. In northern Hungary, now Slovakia, Czech was used by educated Slavs. Those Slavs knew, of course, that they were different from their neighbors, but they lacked any precise idea of their own identity. Peter Brock's brief but erudite and stimulating "essay in intellectual history" describes the emergence of the concept of the Slovak nation. His attention is focused on the thought of leading personalities, but his book is not a pure history of ideas; while no effort is made to place the Slovak problem in a sociological or comparative framework, the author relates individual figures to the cultural institutions in which they

worked. First, in the 1780s, the Catholic intelligentsia of northern Hungary began to use the Slovak vernacular as their written language in place of Czech; politically they continued to regard themselves Hungarian. The Protestants retained Czech, but in a later generation, under the influence of Herder, they developed a cultural-linguistic concept of a "Czechoslovak" nation as an alternative to the political Hungarian nationhood. (This "secession" was only theoretical.)

This was the second phase of the rise of the Slovak national idea. The conditions in Austrian Bohemia and Hungarian Slovakia were too different to make the retention of a common language practicable; in the 1840s the younger generation of "Czechoslovaks" began to use another Slovak vernacular in their popular publications. Slovakia thus acquired a *third* literary language. Finally, in 1851, the Catholics and Protestants agreed on a common tongue, the basis of present-day Slovak. "In theory," says Brock, the making of a Slovak nation was thus accomplished; in practice this did not happen until the fall of Hungary in 1918. Before that happened, assimilation to Magyar was common in Slovakia and the survival of a Slovak nation was at times uncertain. Once Slovakia was separated from Hungary, however, the idea of Slovak nationhood, developed by the early intelligentsia, won the allegiance of the masses.

While Brock's book will interest all students of nationality in Eastern Europe, in this age of ethnic resurgence among the Basques, Catalans, Corsicans, Bretons, etc., it also deserves to be read more widely.

ROMAN SZPORLUK
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MARIUSZ KULCZYKOWSKI. *Chłopskie tkactwo bawełniane w ośrodku andrychowskim w XIX wieku* [Peasant Cotton Weaving in the Center of Andrychów in the XIX Century]. (Prace Komisji Nauk Historycznych, number 38.) Summary in French. Wrocław: Ossolineum, for Polska Akademia Nauk. 1976. Pp. 168. 40 Zł.

This volume continues Mariusz Kulczykowski's detailed study of the social and economic history of Galicia; here he traces the development of the cotton textile industry in the Andrychów area. Replacing the weaving of linen cloth in the 1830s and 1840s as the principal hand-weaving industry, cotton textiles production reached a peak in the 1860s and 1870s. Thereafter it stagnated, to be challenged by the introduction of mechanical equipment in the early twentieth century.

His methodology illustrates how much information and insight can be squeezed from shriveled,

dry sources. In the absence of synthetic archival reports the author relied upon materials such as probate records, property tax lists, church vital records, and oral interviews to recreate the economic and social impact of the cotton textiles industry on the Andrychów region. On the basis of such evidence, Kulczykowski describes the socioeconomic characteristics of this peasant-dominated area.

The micro-level of the study permits the author to give a detailed account of the unequal relationship between middlemen and weavers. Almost two thirds of the distributors were Jewish, of whom some had begun as moneylenders and others as artisans. Landowning "master" weavers employed one to two landless weavers as helpers.

Produced goods have to be distributed, and the book delineates the cotton textiles trade network. The introduction of cotton textiles was accompanied by the replacement of Polish peasant-traders with Jewish merchants as directors of trading operations. The sale of such goods was largely in the hands of "families" (*kolegacje*), or trading companies, employing substantial amounts of capital and operating through teams of traveling salesmen.

Kulczykowski shows that the introduction of cotton textile weaving undermined the linen weaving industry and thus contributed to the further pauperization of the Galician uplands. Thus, the introduction of a potential force for modernization has a destructive impact upon a peasant-industrial community.

Though rich in texture and detail, the book gives no indication of levels of textile production, fails to indicate the broader economic significance of the Andrychów area for Galicia as a whole, and does not indicate whether Andrychów was typical of Galicia. In conclusion, this study, in combination with recent analyses of northeast Russian Poland, adds considerably to our knowledge of provincial Poland's social and economic development.

RICHARD D. LEWIS
St. Cloud State University

ANDREJS JOHANSONS. *Latvijas Kultūras Vēsture, 1710–1800* [History of the Culture of Latvia, 1710–1800]. Summary in German. Stockholm: Daugava. 1975. Pp. 647.

Andrejs Johansons is professor of comparative religions at the University of Stockholm in Sweden, a prolific scholar and a poet. With 647 pages of text, forty-eight tables of rare photographs, forty other illustrations scattered throughout the text, a good summary in German, an impressive bibliography, and excellent indexes, this work can be considered as his best and most ambitious publication to date.

Here he demonstrates his particular interest in the cultural life of the eighteenth century and his great knowledge of Latvian and Baltic German culture. The book reflects both the Latvian indigenous culture, the superimposed Western culture, and the fusion of both these cultures which stimulated the development of modern Latvian culture in the nineteenth and twentieth centuries.

Johansons gives credit to a number of German literary figures and pastors who contributed to the revival of the cultural life of Latvia after it had been terribly devastated during the Great Northern War and the accompanying pestilence. After all of the Latvian provinces were taken over by Russia, the Latvian nation was pushed to the lowest depth of oppression by the Russian government and local German nobles; the Latvian nation nonetheless demonstrated a remarkable strength and will to survive in spite of great odds. In the spiritual survival of the Latvian nation a certain role was played by non-Latvian Moravian Brethren in Northern Latvia and Jesuit fathers in Eastern Latvia. The Age of Enlightenment, however, had the crucial role in the cultural growth of both the Baltic Germans and the Latvians. Some of the most important publications of Immanuel Kant and Johann Gottfried Herder were published first in Riga. Herder's interest in Latvian and Lithuanian folklore gave him the first impetus for his study of nationalism. Latvian folksongs also caught the attention of Sir Walter Scott.

Johansons has done thorough research and his book is well written. In certain places he also demonstrates his fascination with paradoxes, curiosities, and even morbidity. The book lacks a good general introduction to cultural developments and currents in Europe, but otherwise one can find no fault with this great contribution to Baltic history.

One should also express deserved admiration to the Daugava publishing house in Stockholm for its consistent professionalism and extraordinary devotion to high quality work. This company also pays much attention to generally neglected areas of study. Johansons' book is the seventh volume in the monumental series on Latvian history published by the Daugava Förlag since 1958, and it will be followed by several other volumes.

EDGAR ANDERSON
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SERGIUS BOLSHAKOFF. *Russian Mystics*. (Cistercian Studies Series, number 26.) Kalamazoo, Mich.: Cistercian Publications. 1977. Pp. xxx, 303.

The Orthodox Christian churches enjoy a reputation in the West for the large numbers of mystics that have appeared in their tradition. In the East theology and personal experience of God have al-

ways been closely united. Sergius Bolshakoff presents a catalogue of Russians to support this contention.

The book's title is much broader than its contents, for only Orthodox monks and nuns receive consideration. In this regard, the early chapters could very well belong to a history of Russian monasticism. The structure of the work is starkly simple. A short biography of the subject is given, then a list of the person's writings, followed by anecdotes preserved in the community of his or her disciples. The latter are unquestioningly accepted as authentic. The great names of Russian mysticism, such as Seraphim of Sarov and Tikhon of Zadonsk receive more generous treatment, with selections from their most important treatises translated by the author.

Bolshakoff asserts that the distinctive feature of the Russian mystic is a total commitment to a spirituality based solely on Scripture and the Greek patristic tradition. When comparisons are made with Western mystics it is only to emphasize how self-contained the Russian experience really was. Despite the fact that the golden age of the mystic movement occurred as late as the nineteenth century, its main inspiration was drawn from Paisius Velichkovsky's *Dobrotolyubie*, a translation of selected Greek fathers who appeared in the *Philokalia*.

Bolshakoff does not define who he considers to be a mystic until very late in the book. The definition he finally gives is so broad that it allows every monastic spiritual author who lived before the eighteenth century to be so known, ignoring the possibility that a person might well give advice on prayer and the spiritual life without being a mystic.

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S. B. VESELOVSKII, compiler. *Akty russkogo gosudarstva, 1505–1526 gg.* [Documents of the Russian State, 1505–1526]. Moscow: Izdatel'stvo "Nauka." 1975. Pp. 434. 3 r. 31 k.

Despite the fact that some views of S. B. Veselovskii (1876–1952) were unpopular in the Soviet Union, Soviet scholars continue to exhume materials tucked away in Veselovskii's personal archives (Arkhir AN SSSR, f. 620). The book under review represents a continuation of the first volume of *Akty sotsial'no-ekonomicheskoi istorii severo-vostochnoi Rusi* (Moscow, 1952), a collection of documents copied by Veselovskii in the 1920s and 1930s. Like its predecessor, the new collection is composed of sale and gift charters, land surveys, wills, mortgages, and other property settlements which con-

cerned lands either held by the Troitse-Sergei Monastery at the time the document was drawn up, or lands which subsequently came into the monastery's possession.

The basic criterion for including a document—its relation to the Troitse-Sergei Monastery—arises naturally from the arrangement of the Monastery's copybooks where these documents were recorded. Nevertheless, this system proves misleading. As the compilers admit, the texts reproduced here concern lands spread throughout the districts of Rus', but their relation to the Troitse-Sergei Monastery is governed not by the years 1505–26, as the title implies, but by copybooks which were compiled as late as the eighteenth century. The monastery was not nearly so rich two centuries earlier.

Still it is an occasion for joy when any Soviet archival materials see daylight. Most documents printed here are provisioned with detailed commentary, and all are accompanied by manuscript descriptions of the original, lists of surviving copies, and their current locations. Where applicable, variant readings are provided, and if the document was previously published, that too is indicated. All this information is summarized in an appendix of sources. As in Veselovskii's own version, comprehensive subject, name, and geographic indexes are appended.

All Veselovskii's copies are said to have been compared anew with the originals, and several sources unknown to Veselovskii have supplemented the collection. Archeographic commentaries which update the views expressed in the 1952 volume are also useful. Although long in the making, this new publication is a welcome addition to the source base of early Muscovite history.

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NICHOLAS V. RIASANOVSKY. *A Parting of Ways: Government and the Educated Public in Russia, 1801–1855.* New York: Oxford University Press. 1977. Pp. viii, 323. \$25.00.

Those of us who began work in Russian studies during the late 1950s and early 1960s remember well the appearance of Nicholas V. Riasanovsky's book about *Nicholas I and Official Nationality in Russia* which, in the minds of a number of us, established him as an important figure among that handful of scholars who were studying the reign of Nicholas I. *A Parting of Ways* is in the tradition of Riasanovsky's work on Nicholas I and Official Nationality and it sets some of the problems dealt with in that study into the broader framework of relations between government and the educated

public from the time of Peter the Great until the death of Nicholas I.

Riasanovsky first examines the impact of the Age of Reason upon the Russian autocracy and the educated public during the eighteenth century. In his view, "modern Russian educated opinion was created by the reforms of Peter the Great and his successor" (p. 22), and the result was a Russian Enlightenment which embodied certain elements common to the Age of Reason. These included, most importantly, the application of the concept of enlightened despotism to Russia. This latter point is central to Riasanovsky's treatment of the eighteenth century, for he argues that such radical social and political criticism as found an isolated expression in the work of Alexander Radishchev, was "controlled, in a sense, by the concept of enlightened despotism itself, a logical alliance between modernizing rulers and an emerging public opinion." As a result, "Russian government and society followed the path of the Enlightenment in a remarkably united, conscious, and in many ways successful manner" (p. 49).

The French Revolution posed new problems for thinking Russians and for the autocracy itself. Russia's failure to keep in step with the West became a critical problem which, in turn, "made the issue of political and social reform in the empire of the tsars more pressing" (p. 66). The Decembrist Revolt was the ultimate response of the most advanced segment of Russian educated opinion to the government's failure to implement such reforms. Riasanovsky sees that as the final expression of the Enlightenment in Russia, "the best and most far-reaching response made, within the framework of the Enlightenment, by the Russian educated public to the needs of Russia" (p. 96).

The Decembrists' failure deprived educated opinion of further initiative. Thus the period between 1826 and 1840 was marked by calm and order, as educated Russians turned to the more contemplative doctrines of idealism and romanticism. These years saw the full articulation of Official Nationality, but they also laid the groundwork for the Slavophile-Westernizer debate of the 1840s. Riasanovsky argues that the end result was a dramatic shift in relations between educated public and government in which alienation and conflict replaced the unity of the 1830s. Thus, the century-old alliance between the government and the educated public was seriously impaired, perhaps even destroyed, and, in Riasanovsky's view, this was of critical importance.

Riasanovsky concludes that "the split between the Russian educated opinion and the Russian government in the 1840s and 1850s meant in effect the emergence of the Russian intelligentsia" (p. 295), and therein lay the major significance of the

"parting of ways." His description of that complex process draws together much of what scholars have learned about various aspects of that phenomenon during the past several decades. As such, *A Parting of Ways* provides a synthesis of recent Soviet and Western scholarship. Like all syntheses, it has its flaws, and a broader treatment of the interaction between government and educated opinion during the reign of Alexander I might have been preferable to the relatively brief survey which Riasanovsky provides. But the strong points of *A Parting of Ways* outnumber such shortcomings which, after all, are at least partly a matter of preference and emphasis. We have long needed a synthesis of this period. Riasanovsky now has provided one.

W. BRUCE LINCOLN
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JAMES H. BATER. *St. Petersburg: Industrialization and Change*. (Studies in Urban History, number 4.) Montreal: McGill-Queen's University Press. 1976. Pp. xxiii, 469. \$32.00.

H. J. Dyos, general editor of the urban history series in which the volume under review appears, bestows lavish praise upon the book: it contributes "new perspectives" on Russian urban history, is "arduously researched, perceptive," even "magisterial." It is pleasing, for once, to be able to concur with such sweeping claims.

James H. Bater's book is divided into seven chapters, of which chapters 3-6 constitute the core. These chapters consist of a detailed analysis of two periods of St. Petersburg's history, roughly the late 1860s and the years immediately preceding the First World War. Two of these chapters are devoted to each period, with parallel chapters within each pair dealing respectively with economic structure, industrialization, and the transport system (3 and 5) and social structure, hygienic conditions, and demographic patterns (4 and 6). The discussion of the later period also includes systematic comparisons with the earlier, "benchmark" period in order to establish the dimensions of continuity and change. With a few exceptions, the author deliberately excludes the intervening decades from his immediate purview, preferring to employ the method of "cross-sectional" analysis and to infer intervening developments from his comparisons between the two periods. From the historian's point of view something is lost by this procedure. Nevertheless, Bater's approach is justified by the excellent results it yields as well as by the fact that, assuming the same richness of documentation and meticulousness of execution displayed in the present volume, full chronological

coverage would have trebled the length of the book and its time of completion.

Within the constraints of his method, Bater's two periods are well selected. In part they were chosen by political-historical criteria (especially the second period, which enables Bater to evaluate the impact of industrialization on the Petersburg populace on the eve of war and revolution), in part by economic criteria (both are periods of industrial growth), in part by the availability of detailed census materials (1869 and 1910) and factory statistics (1867 and 1913) for these years. Having made these reasonable choices, Bater then keeps political history in the background and concentrates on his examination of the economic and social structure and physical fabric of the city, emphasizing the destructive effects of industrialization (particularly its spatial attributes) and urbanization (broadly defined) on the quality of life of the urban poor. He exploits his command of an imposing array of methodological tools from a wide variety of toolhouses: economic geography (his academic specialization), historical demography, economic history, and just plain history. The book is replete with complex but highly readable tables, graphs, and maps, as well as many handsome photographs.

The types of sources utilized are much too plentiful to list, but the most crucial are: published municipal censuses, city directories (which Bater combines effectively with large-scale maps), and official collections of factory statistics. The few historians of urban Russia who have attempted to work with any of these sources will testify to the extreme challenge they present. It is much to Bater's credit that he has managed to utilize them with enormous technical mastery, while interweaving them with more traditional sources to write an elegant, comprehensible book that should be required reading for all historians of Imperial Russia. Within the framework of the historiographical debate over Russia's progress toward social and political stability on the eve of the war, this book will surely provide grist to the mill of the pessimists.

REGINALD E. ZELNIK
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GIUSEPPE BOFFA. *Storia dell'Unione Sovietica*. Volume 1, *Dalla rivoluzione alla seconda guerra mondiale: Lenin e Stalin, 1917-1941*. Milan: Arnoldo Mondadori Editore. 1976. Pp. 779. L 7,500.

This book deserves a lengthy and major review, and will doubtless get one when it is translated into English—the sooner the better. In scope it

falls between the multivolume work of E. H. Carr and the short surveys by von Rauch, Treadgold, Dmytryshyn, and Ulam, its 780 pages constituting volume one (1917-1941) of a projected two-volume set. Though containing little that is new to specialists, it is an extraordinarily useful and well-documented synthesis of recent and older works, of Western, Soviet, and dissident historiography, of specialized monographs and grand interpretive schemas. Boffa, a "Eurocommunist" special correspondent of *Unità*, is best known here for his *Inside the Khrushchev Era* (1959). His overall positive tone and approach to Soviet history might deepen frowns among some historical cold warriors, but his relentless discussion of controversial matters (particularly of the Stalinist period) will also arouse ire among hard-line Soviet scholars.

There are a number of omissions, odd emphases, and debatable conclusions in this study. Social historians will be disappointed at the conventional treatment of the political parties and the scant attention given to Nepmen, women, Jews, etc. (more space is devoted to the *Smenovekh* episode than to these groups), though Boffa offers some interesting, if spotty, data on "the promoted ones," the professions, and the Komsomol, as well as a lot on peasants and workers. The author passes rather lightly over the Cheka terror of the Civil War without explaining (as Gerson has done recently) that it hit working people at least as hard as it did "class enemies." A long and informative account of the *stalinschina* dismisses the notion that it was "the improvisation of a paranoiac" but fails (as have all other historians) to answer satisfactorily the question of why so many Soviet people killed and were killed. The achievements and the horrors of industrialization and collectivization are both recorded without suggesting what economic alternatives were possible. The author is usually fair to (and reliant upon) Western historians. But in a brief review of the Pipes-Zenushkina (her name is misspelled) argument over nationality policy, Boffa criticizes the Soviet solution (not the best, or even Leninist, he says), praises its cultural and economic accomplishments, but wrongly attributes to Pipes (and, by implication, other Western scholars) the belief that it was "a mere return to Russian imperialism" (p. 214 and n. 41).

The book is far richer than these few remarks and criticisms can convey. It is the best political history of the Soviet Union now available. No archives (except Western ones) were used, but the text and the notes provide an elaborate guide to changing Soviet interpretations and dozens of historiographical digressions on Soviet-Western scholarly controversies. Rejecting compulsive Leninolatry from the outset, the author has made a

strenuous effort to attain a balanced and well-rounded assessment of the first twenty-five years of Soviet history.

RICHARD STITES
Georgetown University

A. A. GRECHKO *et al.*, *Istoriia vtoroi mirovoi voyny, 1939-1945* [History of the Second World War, 1939-1945]. In twelve volumes. Moscow: Voennoe Izdatel'stvo Ministerstva Oborony SSSR. Volumes IV, V, VI. 1975; 1975; 1976. Pp. 532; 508; 518. 2 r. 80 k. each.

In these volumes, Soviet historians continue their massive effort to expose "bourgeois falsifications" of the history of the Second World War. Volume four discusses the beginning of the "Great Fatherland War" for the Soviets, marked by the German invasion of the Soviet Union in June 1941. The marriage of convenience between the Western allies, Great Britain, and the United States, and the Soviet Union becomes the "anti-fascist coalition" and the war becomes a "just" war from the Soviet standpoint. But the Soviet authors are careful to make clear that the war aims of the USSR differed from those of its bourgeois partners. The latter sought merely to eliminate Germany, Italy, and Japan as economic competitors, hoping at the same time that the Soviet Union would be severely weakened. The Soviets, on the other hand, were seeking the more noble aims of defending the great gains of socialism, rendering assistance to peoples who had fallen under the fascist yoke, and defending world civilization.

The initial Soviet defeats are ascribed to a number of temporary advantages enjoyed by the German Wehrmacht at the start of the war: the availability of the military resources of all of Western Europe; the lengthy period Hitler had to prepare the attack; the combat experience of the German forces; and, their high-level of mobility. The achievement of surprise enabled the Germans to disorganize planned Soviet defensive deployments and forced the Soviets to commit reserves before they were fully concentrated. The inexperience of many large unit commanders also contributed to the early defeats. The main reason for this shortage of experienced commanders—the military purges of 1937 and 1938—is not mentioned.

The apportionment of blame for the defeats is treated impersonally: "... miscalculations in defining the possible timing of Germany's attack on the Soviet Union and, linked with them, negligence in preparations to parry the first strikes of the aggressor played their role" (p. 58). There is no mention of those who were made scapegoats and shot by Stalin such as D. G. Pavlov, the commander of the Western Front.

For the specialist, there is little that is new in the account of the initial battles and the reaction in Moscow. The question of Stalin's role in the early days of the war is left unresolved. The very day of the attack, 22 June 1941, someone (we are not told who) dispatched the chief of the General Staff (Zhukov), his principal deputies and others to the front to find out what was happening (p. 37). But later the same day, the Main Military Council issued an order signed by Timoshenko, the Minister of Defense, countersigned by Malenkov (acting in what capacity we are not told) and Zhukov, ordering counterattacks all along the front (p. 38). The next day, the *stavka* (headquarters) of the High Command was formed with Timoshenko as its chairman and Stalin merely a member. On 30 June, Stalin became head of the State Defense Committee (GKO), but it is not until 10 July that Stalin also became chairman of the *stavka*. No explanation of this curious sequence of events is attempted.

Volume five focuses on the German 1942 offensive that advanced to the gates of Stalingrad and to the Caucasus. Stalin refused to accept the advice of the General Staff to remain on the defensive and to prepare for a general offensive in the summer of 1942. Instead, he authorized an offensive toward Kharkov in May, almost coinciding with the beginning of a German offensive toward the southeast, which took the attacking Soviet forces in the flank and rear, and facilitated the German summer advance. In his famous "secret" speech to the Twentieth Party Congress in 1956, Khrushchev claimed that Stalin refused to listen to his pleas to call off the offensive. Since Khrushchev's removal, the official line is that Stalin was receiving information which contradicted Khrushchev's reports, and the Soviet attack was allowed to continue. This version is the one used in the current volume.

The credit for proposing the counteroffensive at Stalingrad, the turning point of the entire war according to these volumes, is assigned to Zhukov, Stalin's troubleshooter, and Vasilevskii, the chief of the General Staff. While alive, Stalin took full credit for the plan; after his death, Khrushchev and Yereimenko, commander of the Stalingrad Front, tried to claim it, but on page 27 of volume six we are told that Stalin made the principal decision on 13 September 1942 on the advice of Zhukov and Vasilevskii.

The efforts of Great Britain and the United States to do their part in the war against the Axis are also reported in these volumes primarily, it appears, to provide counterpoints of ineffectiveness, timidity, and deviousness to Soviet military skill, gallantry, and determination.

WILLIAM J. SPAHR
Alexandria, Virginia

ROY A. MEDVEDEV and ZHOES A. MEDVEDEV. *Khrushchev: The Years in Power*. Translated by ANDREW R. DURKIN. New York: Columbia University Press, 1976. Pp. xi, 198. \$10.95.

This book is a minor monument of ambiguity. One of the two authors, Roy Medvedev, the prominent dissident-historian, remains in Moscow, whereas his brother and co-author, Zhores (Jaurès) Medvedev, is in the West and arranged for the book's publication in English. No less ambiguous is their judgment of the Khrushchev years—the era of de-Stalinization and beginning détente, yet also of economic, administrative, and political blunders of the first order, as well as frequent chaos and zig-zags in Soviet politics. As they write, "We participated in the hopes and disenchantments of the period; we experienced enthusiasm and bitterness, elation at his bold political and diplomatic reforms, and exasperation at his sometimes startling ignorance when it came to handling simple economic, agronomic, and theoretical problems" (p. ix).

Their discussion of the Khrushchev era—frequently based on participant observation or oral reports by witnesses they interviewed—is always informed but necessarily episodic and often skimpy. For Soviet citizens the authors have remarkable perspective and detachment, but they would scarcely claim that this is a definitive study. It is in fact less satisfactory or significant than Roy Medvedev's *Let History Judge! or On Socialist Democracy*, or than Zhores Medvedev's *Rise and Fall of T. D. Lysenko*.

Their treatment of post-Stalin elite politics by and large coincides with other available accounts. In regard to the crisis of 1957, the authors make a good point in stressing that the provincial secretaries who saved Khrushchev in the Central Committee were not so much his personal cronies as the beneficiaries of the restored role of the Communist Party. In dealing with the "rehabilitation" of Stalin's victims they unwittingly underscore the qualitative difference between the Stalin and Khrushchev eras when they report that according to the best estimates seven to eight million people (elsewhere they say eight to nine million) were released from labor camps, and another five to six million posthumously rehabilitated (pp. 20, 74).

It may be significant for the perspective and priorities of Soviet observers that the authors' least satisfactory treatment is of foreign relations (including China and Eastern Europe). Instead, they focus on agricultural problems and concerns, including Khrushchev's earnest improvisations, false starts, and phoney targets. Unusual in this connection is the Medvedevs' conclusion that the dissolution of the system of machine-tractor stations may have been a mistake. Moreover, the authors

provide some revealing and novel details about the so-called "Riazan' fiasco" (pp. 94–101).

The authors have little to say about the political crises of the early 1960s or about the dynamics of Khrushchev's own behavior, including the changes over time in his style, priorities, and perceptions. They are inevitably unfamiliar with Western studies of the same period, but their judgment—even where controversial—is serious and sound. It is all the more regrettable, then, that this book could not appear in the Soviet Union where it is needed more than it is in the West.

ALEXANDER DALLIN
Stanford University

WILLIAM M. MANDEL. *Soviet Women*. Garden City, N.Y.: Anchor Books, 1975. Pp. 350. \$3.50.

William M. Mandel's work on Soviet women was not written primarily for a scholarly audience. It has an abbreviated bibliography, no footnotes, and, more fundamentally, it is descriptive rather than analytical. For analysis of the history of women in Russia there are richer studies available from H. Kent Geiger, Gail Lapidus, and Richard Stites, to name only a few.

Mandel's book does have value, however, for it paints a comprehensive picture of the life of Soviet women today. Mandel is not obsessed by statistics, and he is blessed with broad contacts among urban Russians that add welcome insights into individual attitudes. The author is also an unabashed admirer of the Soviet Union, thus he praises, with qualifications, the genuine progress that nation has made toward female emancipation. Western feminists and scholars have been quick to criticize the Communist Party for compromising the liberating vision held by the first generation of Bolsheviks. Mandel rightly asserts that "... the question is only whether to regard anything short of complete equality as meaningless, or to consider the cultural and historical background in forming our judgments" (p. 304).

The author's sympathies, however, lead him into overstatement which must be approached carefully. It is inaccurate, at the least, to write that Soviet trade unions are democratic or that the Supreme Soviet is the USSR's equivalent of the U.S. Congress (p. 301, 172). The unions have not been democratic since the Civil War, and the Supreme Soviet possesses no real power. Such exaggerations are glaring; less obvious and more relevant to women's history is Mandel's refusal to deal with the economic motives behind the party's commitment to female emancipation. He believes that altruism was the primary factor impelling Communists to improve the status of women, when in fact the need to mobilize the entire population for

industrialization played an equally important role. In fact, the party has ordered its priorities to serve industrialization, and underfunded social services, a decision that continues to affect women today. By ignoring the government's choices, Mandel oversimplifies the history of female emancipation in the USSR and idealizes the present, thereby distorting the determinants of the current position of Soviet women. Nevertheless, his book can be read with profit, if read cautiously.

BARBARA EVANS CLEMENTS
University of Akron

NEAR EAST

STANFORD SHAW. *History of the Ottoman Empire and Modern Turkey*. Volume 1, *Empire of the Gazis: The Rise and Decline of the Ottoman Empire, 1280-1808*. New York: Cambridge University Press. 1976. Pp. xiii, 351. Cloth \$23.50, paper \$12.95.

The military and territorial expansion and retrogression of the Ottoman Empire over the first five hundred years of its history is the main theme of Stanford Shaw's courageous and commendable effort at writing a general book in the field. (The work's value will be much enhanced by reading it in conjunction with D. E. Pitcher, *An Historical Geography of the Ottoman Empire* [1972].) The author is the doyen of American-trained Ottoman historians (he was a student of the late L. V. Thomas of Princeton) and the American pioneer in the extensive utilization of Ottoman archival sources in monographic literature. In the preface and on the dust jacket both author and publisher assert that the work is based on Ottoman archival sources. In the only part of the text which appears, through footnote citations, to rely directly on the Ottoman archives, I found the effort to be, at best, a duplication of a similar though more detailed and earlier study by a modern Turkish historian. (These sections are covered by the first sixty-six footnotes of chapter 7, "New Challenges and Responses, 1683-1808." The latter part of the same chapter and the period of time it covers are in the main based on Shaw's monograph detailing the reign of Selim III. The aforementioned Turkish work is I. H. Uzünçarşılı, *Osmanlı Tarihi*. IV;1 [1956]: 8, 10, 70-71, 93, 99, 132, 133, 135, 175, 177, 180, 181, and 193.)

The overall orientation of the book lacks any conscious theoretical framework, but this does not imply that Shaw's values, sympathies, and historical premises are not reflected here. The book is written from a legitimist point of view and therefore focuses on the decisions, policies, and personal histories of members of the Ottoman dynasty as the proper problems for historical study. The sov-

ereignty of the dynasty is, as a consequence, considered immutable; any historically evolved movements (be they popular, impersonal, or even external) which threaten to abridge that sovereignty are considered ill-founded. On one level this type of interpretation is quite useful, since it makes drama out of history, and hence the stuff of history is made the more palatable for the student. As a system of explanations, however, it is not only rigid, narrowly based, and ahistorical but also tends to equate change with chaos. For example, political conflict is nowhere treated as a sign of a dynamic and creative society. Instead, social forces which challenge (and try to share power with) the dynasty are portrayed as nuisances to be destroyed with impunity. When successful, however, these challenges are seen as symptoms of decay. Sultans, other individuals, and groups are swayed (and motivated) not by *raison d'état* or self-interest, but in the main by such ahistorical drives as religious affiliation.

The major difficulty with Shaw's approach is its incapacity to accommodate different and new scholarship. Despite the heavy reliance on a large bibliography of modern scholarship for the writing of the book, new ideas sit uncomfortably side by side with the old, neither affecting them nor affected by them. His essential framework—basically a philosophy of history of the rise, decline, and fall variety—remains, in the end, unmodified.

RIFAAT ALI ABOU-EL-HAJ
California State University,
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JOHN P. ENTELIS. *Pluralism and Party Transformation in Lebanon: Al-Kata'ib, 1936-1970*. (Social, Economic and Political Studies of the Middle East, number 10.) Leiden: E. J. Brill. 1974. Pp. xi, 227. 68 gls.

It is extremely difficult to write a review of a book on the Lebanese Kata'ib Party when the reviewer has the benefit of hindsight of the Lebanese war of 1975-76 and the author did not. If John P. Entelis could have written his monograph after the Syrian intervention of 1976, he would undoubtedly have changed a great deal in his analysis of the Phalangist Party. Despite this handicap and notwithstanding the traumatic effects of the civil war on Lebanon and the Kata'ib Party, Entelis' book is extremely valuable for an understanding of the foundation, growth, and ideological transformation of the Kata'ib Party between 1936 and 1970.

Entelis begins his work by describing the reasons for the founding of the Phalangist movement. They were to inculcate in the youth of Lebanon a sense of Lebanese nationalism, with a Christian bias, and a desire for independence from France. The Phalangists were closely identified with the

struggle for independence, and when this goal was achieved in the 1940s, the Kata'ib suffered an identity crisis since it no longer had a *raison d'être*. An alternative to dissolution was found in transforming the former paramilitary organization into a mass-based political party. This transformation of the Kata'ib, the author argues, was successful. The party put forward candidates for elective office and ran on a platform of "Lebanonism"—insistence that Lebanon was a unique nation which was not exclusively Arab. "Lebanonism" carried with it a continuing Christian bias just as Arabism had an Islamic bias. The author then proceeds, once he has examined the emergence of the party's ideology, to enlighten his reader about the Kata'ib's organizational structure, membership, and leadership patterns, and how a political party operates in a confessionally pluralistic society run by traditional elites. (All students of the Middle East are intrigued by Arend Lijphart's model of consociational democracy as it applies to Lebanon.)

The author argues that many non-Maronites adhered to the Kata'ib definition of Lebanon and that as a result the party flourished, becoming by the late 1960s "the most interconfessionally represented" of Lebanon's political groups. Entelis argues that the Phalangist Party was a force for change; it advocated laws designed to rectify glaring social and economic inequalities and supported social security legislation.

Entelis' book is generally the story of a success. But we now know that the slow process of changing the Kata'ib Party from an almost exclusively Christian organization into a secular political party halted with the recent civil war. In fact the process has been reversed as the sectarian Hydra has once again been revived in the recent fighting. The Kata'ib Party seems to have reverted to its 1936 mentality. Why has this occurred—or has it? Was the change from a sectarian to a secular stance in the Kata'ib Party real or imagined? Why has the Phalangist organization recently identified itself with a more parochial definition of Lebanon and even encouraged talk of religious partition of the country? All of these questions should be broached by whoever writes the sequel to Entelis' book.

WILLIAM W. HADDAD
Illinois State University

JOHN F. DEVLIN. *The Ba'th Party: A History from Its Origins to 1966*. (Hoover Institution Publications, number 156.) Stanford: Hoover Institution Press. 1976. Pp. 372. \$11.95.

The changeover in Syria and Iraq from traditional, patriarchal governments to highly politicized and ideological regimes was violent, sudden, and unfam-

iliar in its development. The two countries' present rulers come under the same rubric, Ba'thists; in this book, John F. Devlin studies the Ba'th Party from its origin to 1966. Having served in the American Embassy in Baghdad and in the Consulate in Kuwait the author then joined the CIA in 1960. He is now the deputy director of the agency's Office of Political Research. Devlin, however, does not use any classified information available to him because of his employment.

The year 1966 marks the great schism which split the party into two antagonistic organizations differing not only from each other but also from the original party in its formative years in Syria. One branch remained in Syria, the other implanted itself in Iraq which had experienced Qasim's bloody seizure of power in 1958 and his assassination in 1963. The founders of the party themselves and notably Aflaq (portrayed as unworldly but who now lives in a government palace in Baghdad) were expelled from Syria. The grandiose vision of Arab unity as the first objective toward complete emancipation from foreign domination was thus shattered; antagonisms between parties, peoples, and states resumed their traditional supremacy. The rubric *Ba'th* remained, however; the question is how and why.

Starting with a description of the ideology of the party, Devlin goes on to tell an almost day-to-day story of the events, a story which will be bewildering to the non-specialist but which amply illustrates the author's most significant conclusion. The party, civilian in its origin, became powerful only when the military came to dominate it. In his opinion—with which it is quite impossible to disagree—the development of the *Ba'th* merely parallels "the general line of development in the Arab world since World War II, a period which has witnessed the substitution of authoritarian military-based systems for Western-imported representative government."

SYLVIA G. HAIM
London, U.K.

H. H. BEN-SASSON, editor. *A History of the Jewish People*. Cambridge: Harvard University Press. 1976. Pp. xii, 1170. \$40.00.

This huge collection of essays by "leading scholars at the Hebrew University, Jerusalem" brings to ultimate expression the "peoplehood-and-history" theory of Jewish historiography. The theory presupposes that a single, clearly defined entity, "the Jewish people," has produced a unitary and linear history, "Jewish history," which extends back to the time of Abraham. It draws together into one cultural, economic, and social continuum all the doings of Jews everywhere at any one time. So, on

pages 468–69, H. H. Ben-Sasson gives us “Jewish Participation in the Reconquista Colonization,” “Jewish Commerce in the Indian Ocean,” “Jewish Economic Activity in Christian Spain,” “The Livelihoods of Jews in the Byzantine Empire,” and “The Transition to Money-Lending in Ashkenaz”—in five contiguous paragraphs! Moreover, the theory takes for granted that certain (unspecified) traits are everywhere and always definitive of what is “Jewish.” This last view culminates in the following judgment: “It is not surprising that many people, both Jews and gentiles, friends and enemies, have continued in modern times to occupy themselves with the riddle of the endurance, survival, and renaissance of the Jews. They ask why larger groups and equally strong cultures and civilizations—the Persians in Moslem culture and the Greeks in Roman-Byzantine culture—have vanished or have been absorbed, *while the Jews have remained Jews*” (p. 728, italics mine). In fact, the distinctive history of Iran within Islam, set apart in both language and theology, has left the contemporary Iranian in far closer touch with pre-Islamic Iranian civilization than is the contemporary Jew of New York or Tel Aviv with the Talmudic world of pre-Islamic Babylonia.

But more to the point, the linear history follows a highly conventional, even canonical, definition of which Jews at a given time were engaged in the making of “Jewish history,” Jews in Islamic countries, for example, scarcely participate in “The Modern Period,” and those in the Greco-Roman and Iranian diasporas of late antiquity appear for only perfunctory paragraphs. Moreover, the authors stress political and institutional history (and, for the first two millennia, military history), while religion and literature are deemed mere curiosities. After nearly two hundred pages of biblical history, with intense attention to archeological materials but only episodic interest in literary history, after extended accounts of invasions, international politics, and court intrigues, the reader’s obvious questions—what has happened to the prophets? where is cult? did not the Old Testament come out of this period?—are answered as follows: “Neither the history of the religion of Israel nor the history of biblical literature have [*sic*] been included, as they are subjects adequately treated by biblical scholarship” (p. 181).

Also excluded is the massive body of scholarship on the historical sources of the post-biblical period, down to Islam. The account of “The Period of the Second Temple” is essentially a paraphrase of Josephus, and “The Era of the Mishnah and Talmud,” predictably, is described principally by innocent narration of Talmudic fables about rabbis.

Since “a history of the Jewish people” from the perspective of Jerusalem culminates in the creation

of the State of Israel, it is no wonder that the modern period (but only the modern period in Europe and in Palestine) predominates. Still, the disproportions are genuinely surprising. The 1300 years from 640 to 1973 occupy 64 percent of the book, and the 2300+ years from Abraham (supposedly about 1700 B.C.) to 640 get 36 percent. True, the disproportion is natural, once political and institutional history is placed at the center, for much more is known about this kind of history in the medieval and modern European settings than in the ancient Middle Eastern one.

Still, even the discussion of medieval and modern Europe is confused. Ben-Sasson decides to treat Jews in both Islam and Christendom together—they are, after all, “one people”—with the result that “The Economic and Social Crisis in Christian Spain” stands cheek by jowl with “Jewish Settlement in Eastern Europe and the Barrier of Muscovite Russia” (pp. 568–71). The utter chaos produced by the unitary and linear theory of “Jewish history” is perhaps the book’s best demonstrated proposition. In fact, it is the only really clear and well-documented one, since Ben-Sasson has not provided an introduction, explaining the purposes and methods of the work. Nor does the book contain a single footnote!

The fact that the contributors are deemed to be leading scholars in their fields gives this work its peculiar importance as an exceptionally clear and detailed example of what the “peoplehood-and-history” theory makes of “the Jewish people” and of “Jewish history.” But it also demonstrates the limitations of this approach. Viewed separately, some of the essays are valuable. Three of them, S. Ettinger’s *The Modern Period*, A. Malamat’s *Origins and Formative Period*, and H. Tadmor’s *The Period of the First Temple*, are superb.

JACOB NEUSNER
Brown University

NEVILLE J. MANDEL. *The Arabs and Zionism before World War I*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xxiv, 258. \$15.00.

The conclusion of this work, that Arabs and Zionists were at odds well before World War I and the Balfour Declaration of 1917, will not surprise those familiar with Palestinian history. Neville Mandel’s discussion of Arab-Zionist relations, however, is an important contribution. Originally a St. Antony’s thesis, the book is now a clearly-written exposition of the reaction of Arab elites in Palestine, Syria, and Egypt to the growth of the Palestinian Jewish community and the Zionist movement. Since the approach is chronological, readers will find some difficulty in tracing the response of particular political or intellectual figures such as Négib Azoury, Rashid Rida, or Najib Nassar

(leading anti-Zionist Arab publicist in Palestine), but Mandel's arrangement highlights the growing tension between the two communities.

Much attention, however, is necessarily given to Ottoman policy (largely a recapitulation of Mandel's articles in *Middle East Studies*); there are actually three parties to the struggle, indeed four if the capitulatory powers are included, and to that extent the book is wider in scope than the title indicates. Ottoman authorities were clearly in a dilemma: Abdülhamid and the Young Turks alike opposed Jewish immigration but had little power to block the entrance or settlement of foreign nationals. By the eve of war, relations were more complex, and talk of a possible Arab-Jewish understanding offered a glimmer of false hope for the future. Although Mandel begins in the early 1880s, half the book is devoted to the period 1909–14. It is in this detailed section that his principal point is proven. Before 1914, Arab anti-Zionism had emerged, a growth rooted in local patriotism, Arab nationalism, and Ottoman loyalism. Anti-Western xenophobia played a role, but so too, paraically, did the impact of European anti-Semitism.

Mandel's diligent research is commendable and enviable. Arab sources are scanty, but he has mined European, Turkish, and Israeli collections, above all the Central Zionist Archive (Jerusalem) which provides, among other things, a systematic survey of the Arabic press after 1911. Students will find the "Notes on Sources" and bibliography useful in this connection. The book is burdened with too many typographical errors, and an index which lists under "A" every name, place, or title prefixed with the definite article "al" is unacceptable in a scholarly work of this sort.

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AFRICA

J. FORBES MUNRO. *Africa and the International Economy, 1800–1960*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 230. Cloth \$12.00, paper \$5.75.

This application of comparative historical method yields a largely successful synthesis of the modern economic history of sub-Saharan Africa. J. Forbes Munro has attempted to write an empirical history, rather than apply any of various theories of development and underdevelopment. In fact, he implicitly adopts a general equilibrium economic framework, coupled with a liberal political outlook. The strength of this approach is that Munro gives proper attention to the role of economic factors, not just social or political policy, in causing African economic change. He investigates the continent through the variables of population, volume

and value of overseas trade, investment, and the degree of state economic regulation. The chapters are chronological, often with a regional breakdown showing the varying regional changes in these main variables. The statistical work is based primarily on statements of foreign and domestic trade published annually by the United Kingdom.

This is a sound introduction to African economic history. As Munro recognizes, there is danger in writing a general economic history of Africa when so many monographic works are yet unwritten or undigested. His study, however, is a careful summary of the main outlines currently known. It is written clearly and precisely, giving fair summaries of major developments and controversies, and it includes many stimulating inductive historical generalizations resulting from his comparative approach. For example, Munro shows how East Africa was transformed from an economic periphery of India in precolonial times to a periphery of Britain in the colonial period, and then to a periphery of the whole international economy following independence. And in several sections on big business, Munro includes timely reminders of the long-standing significance of U.S. investment in South Africa, Rhodesia, Zambia, Zaire, Angola, and Liberia.

Munro's central theme, "the integration of Africa into, and the subsequent structural shaping of African economies by, the modern international economy," is implemented faithfully throughout the text. This "center-periphery" approach—that is, European and American causes for African events—may in fact be the dominant aspect of modern African economic history. But African causes of African events were also of great significance, and the evidence supporting this alternative approach will grow as current research is published. (This will include studies of political economy, giving more attention to social structure in Africa and elsewhere.) Nevertheless, Munro has given an effective presentation of the operation of the international economy. It may be particularly recommended to historians of the Euro-American "center," so that they may learn about African causes of European and American events.

PATRICK MANNING
Cañada College

KENNETH L. BROWN. *People of Salé: Tradition and Change in a Moroccan City, 1830–1930*. Cambridge, Mass.: Harvard University Press. 1976. Pp. xvii, 265. \$18.00.

Kenneth L. Brown's *People of Salé* is an extensively researched, well-written, and delightfully detailed study of a North African city between 1830 and 1930. The author has used Arabic manuscripts,

including the private family papers of inhabitants of Salé and public documents, European colonial and diplomatic sources, field research and interviews, and an extensive list of secondary studies on specific Moroccan subjects and on the theory of urban history. He has divided his study into four parts, one each on market and town, the fabric of society, the disruption of economic life with the French takeover in neighboring Algeria in 1830 and after, and reflections of, and reactions to, change among inhabitants of Salé in response to the European presence in their own country.

There are so many interrelated theses presented and effectively explored that it is almost impossible to describe briefly the main arguments offered in this book. Brown shows, for example, how the Slawis (the elite inhabitants of Salé) tried to maintain their religious and cultural heritage while at the same time attempting to adapt to economic and political changes. Many did not adapt quickly enough and became impoverished. Others replaced them among the city's elites. The author also discusses the existence of social classes of sorts, although there was little class consciousness or class conflict. Overall, the Slawis safeguarded traditional values—even in the 1930s, religion and nationalism were closely allied in the Moroccans' opposition to cultural assimilation by the French—thus permitting the precepts and reality of a "community bound together by a common culture and social ties" to be maintained. These traditional values which allowed the Slawis to resist French political and economic dominance finally began to break down during the twentieth century as the young among the elite began to ally with families in other cities, as Salé began to become what it is today, essentially a suburb of Rabat. Local allegiances thus finally began to give way to national feelings, although they were not destroyed.

Thus, Brown places the specifics of Salé's history in a larger Moroccan setting, with frequent references to developments in other Maghribi cities, and in the context of theoretical works on urban history. *People of Salé* is, in part, a history of French colonialism as perceived by the colonized, in part, an excellent explanation of how important Islam is and of how it affects individuals and specific communities in North Africa. Brown's book is, in short, an exciting book that many—historians, geographers, political scientists, sociologists, and others—will want to read more than once. It will certainly help those who teach courses dealing with North Africa or the Middle East to explain to students exactly what it is to be a Moroccan, an urban North African, and a Muslim in the modern world.

ALF ANDREW HEGGOY
University of Georgia

EDMUND BURKE III. *Prelude to Protectorate in Morocco: Precolonial Protest and Resistance, 1860–1912*. (Studies in Imperialism.) Chicago: University of Chicago Press, 1977. Pp. xxii, 306. \$18.50.

As historians and political scientists examine the course of European imperialism in the third world, it is perhaps inevitable that there will be revisionists who find the works of preceding authors inadequate and dated. If history is to be a living, vital field, this is desirable and welcomed. Edmund Burke III, in dealing with the conquest and annexation of Morocco, is a self-proclaimed revisionist who refuses to accept many of the earlier interpretations as telling the full story of France's efforts in the western Maghrib. Burke's *Prelude to Protectorate in Morocco* takes the critical years from 1860 to 1912 and examines them from a Moroccan standpoint. His work merits praise for his serious, scholarly, and dispassionate study of precolonial protest and resistance against the slow encroachment of the European powers.

Burke has broken loose from the traditional methodology of studying the imperial phenomenon by trying to show the stresses and strains within Morocco during the imperial period. This is no attempt to make more of Moroccan resistance efforts than there really was, and Burke tells us that many Moroccans will not agree with his efforts. The author convincingly argues that there were three major forces of change within Morocco which eventually combined to break down the traditional rule of the sultan and bring on the final agony of Morocco in 1912. The influx of European goods and money, the failures of internal reforms, and the massive efforts of the French and of the Spanish brought about an irresistible force for alteration which in the long run mortally wounded the native government.

The author's emphasis on the impact of European trade and capital certainly goes hand in hand with the important new work by Kenneth L. Brown, *People of Salé: Tradition and Change in a Moroccan City, 1830–1930*, showing that great changes were under way before 1900, when we can mark the start of French efforts to move into Moroccan territory. In the area of Moroccan sponsored reforms the historian is reminded of the sad end of the reforms of Ahmad Bey in Tunisia. As Leon Carl Brown has stated in his *The Tunisia of Ahmad Bey*, internal reform in a North African state was an extremely difficult thing to achieve. Burke, like Brown, correctly sees merit for Morocco in these attempts at reforms, despite their failure at that particular point in history. To see this attempt to reform as a peculiar Moroccan problem would be a serious error, and it is to Burke's credit that he avoids any attempt to do so. This reader sees Moroccan problems, not only in the context of

Maghribi development, but also as a part of the entire Middle East.

In many respects Burke demolishes the idea that Moroccan development began with the French annexation in 1912. Anyone familiar with his articles knows his dedication to serious scholarship, and this is again reflected in the detailed research done for this important work. The bibliography is an impressive list of almost all of the major archival sources and pertinent official records of the period. Based on the best of historical method, this work is well written, logical, and coherent.

There is a tendency to underplay at times the French colonialists and their role in the annexation of Morocco, but when the reader considers the announced scope of Burke's work, this tendency is understandable. As a part of the growing literature on internal Morocco, *Prelude to Protectorate in Morocco* must stand as a valuable contribution, well presented and well argued. The book is, and will remain, an excellent source, in English, on the western Maghrib prior to the Treaty of Fez of 1912.

JAMES J. COOKE
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BRUCE FETTER. *The Creation of Elisabethville, 1910-1940*. (Hoover Colonial Studies.) Stanford, Calif.: Hoover Institution Press. 1976. Pp. xx, 211. \$8.95.

The copperbelt of Central Africa spans Shaba (Katanga) in Zaire and the adjoining area to the south in Zambia. Historians are intrigued by the past interaction there of big business and colonial government. Social scientists are attracted by the consequences of urbanization in the area, where a network of industrial towns developed and many thousands of Africans live completely or partly divorced from the traditional rural way of life. Bruce Fetter has drawn these two themes together in a compact narrative account of Katangan history from 1910 to 1940, which takes the establishment of Elisabethville (renamed Lubumbashi in 1967) as its focus.

The Union Minière du Haut-Katanga, incorporated in 1906, came to dominate the colonial economy as the copper industry prospered. Elisabethville, the site of the company's smelter, connected by rail to South Africa, and close to the Star of the Congo mine, was destined for a position of exceptional importance in the Belgian territory's affairs. It was also 1,200 miles from the administrative capital, Boma: the origins of Katanga secession are sometimes indicated and often implied in the course of the book.

In the early years of Elisabethville's growth the administration's energies were directed at warding off a (largely imaginary) British threat from across

the border in Northern Rhodesia, while the Union Minière was intent on establishing reliable sources of African labor. Conditions at the first labor camps were appalling. The death rate for Africans in the city was twenty-four percent in 1911. By 1939 it had dropped to two per thousand at the Union Minière Lubumbashi camp. Africans then had the choice of living in comparatively good conditions under strict control at company camps, or in inferior quarters with greater personal freedom in the *cité indigène* of Elisabethville.

Fetter describes in detail the interlocking activities of the Belgian colonial "trinity of power"—government, business, and the Catholic Church. His narrative is sometimes disjointed, but perhaps no more so than the colonial policies of the period. Sections on the affairs of each of the three partners are dovetailed with accounts of corresponding developments in African affairs. In some places further comparison with neighboring governments or with mining policies in Northern Rhodesia and South Africa would have underlined the significance of events in the Belgian territory. In labor matters, for example, the Union Minière followed a very distinctive course. Fetter notes that white South African workers, considered dangerously militant, were deliberately excluded from the mines at an early date. The temperament of the predominantly Belgian skilled labor force was very different from that of white industrial workers in Northern or Southern Rhodesia, where the South African influence prevailed. The Union Minière's efforts to stabilize its African labor force in the 1920s had no equivalent at the Northern Rhodesian copper mines until after the Second World War.

Students of colonial rule, labor history, and urban development in Africa will find Fetter's book of considerable interest. It will be particularly useful to those not familiar with his French-language sources.

ELENA L. BERGER
Journal of Economic History

OLAJIDE ALUKO. *Ghana and Nigeria, 1957-70: A Study in Inter-African Discord*. New York: Barnes and Noble. 1976. Pp. 275. \$17.50.

From the moment they became independent Ghana and Nigeria were manifestly destined to play major roles on the African continent, albeit for different reasons. As colonies of the same imperial power, they had acquired a common official language and common legal, administrative, and educational systems. Members of the modernizing elites in both countries had attended the same schools, shared parallel experiences in Britain or America, and had been involved in the same move-

ments, from the Pan-African congresses to the West African Students' Union. Ghana and Nigeria were also the two most valuable segments of England's tropical African empire. It might have been expected, therefore, that the two countries would seek to develop close cooperative links. In fact, they began to move apart even before Ghana's independence, and by 1970 both had virtually dismissed the hope of cooperating with each other.

Olajide Aluko's book is an attempt to identify and to analyze the main sources of disharmony in the Ghanaian-Nigerian relationship. After tracing the decline of cooperation and the breakup of common institutions (such as the West African Currency Board or the West African Frontier Force), which had already begun in the 1950s, the author focuses on the two countries' rivalry for leadership in Africa and on their contrasting attitudes toward the great powers. Ghana's claim to leadership was naturally based on the fact it had been the first colonial territory in black Africa to achieve independence, but it derived additional credibility from Kwame Nkrumah's militant commitment to the twin causes of African liberation and unity. By contrast, Nigeria's expectations were solely based on that country's size, population, and resources, which, as Nigerian politicians never tired of repeating, made it "the giant of Africa." Apart from such boasts, however, Nigeria's leadership role during those crucial years was all but non-existent.

But the most startling aspect of the uneasy relationship between Ghana and Nigeria is the way in which the two countries came to exchange their positions over major issues after the two military coups which, in early 1966, successively toppled the Nkrumah and Balewa governments. In Ghana, the military-police government headed by General Ankrah and Colonel Afrifa, as well as its civilian successor under Kofi Busia, virtually surrendered all claims to a significant role in African or international affairs and came to espouse American positions on such issues as Vietnam or China's admission to the UN—not to mention Busia's endorsement of South Africa's offer of a "dialogue" with black Africa, which caused embarrassment even among his own cabinet. Meanwhile, faced with the threat of Biafran secession (assisted more or less overtly by France, Portugal, and South Africa), the Federal Military Government of Nigeria noted with some irritation that the United States was evading any concrete commitment to the cause of Nigerian unity while the Soviet Union provided the federal side with substantial military assistance. Nigeria's foreign policy stance, which had been overwhelmingly and even militantly pro-Western prior to 1966, now evolved toward a more balanced form of non-alignment. At the same time, the spectacular de-

velopment of its oil resources equipped Nigeria with the means of exerting a measure of influence more commensurate with its size and ambitions.

In January 1972, however, the Ghanaian military overthrew the Busia regime and over the past five years, the National Redemption Council led by General I. K. Acheampong has been steering Ghana toward a foreign policy line more reminiscent of Nkrumah's "moderate," pre-1961 period. Aluko, whose study covers only the 1957-1970 period, deals only briefly with this last development and cautiously suggests that the chances of an improvement in Ghanaian-Nigerian relations "are now better than at any other time since their independence."

Readers familiar with African affairs will learn little from Aluko's book, except perhaps for his chapter on "Ghana and the Nigerian Civil War." For the rest, despite its impressive array of some 1,300 footnotes, the book is essentially a compilation of known facts, culled for the most part from the West African press. The author's level of analytical sophistication seldom rises above that of the editorial comments written at the time of the events he relates. This contributes to the reader's sense of *deja vu*. This is unfortunate because Aluko's book, though uninspired, is on the whole well organized and competently written. It might have been a better work if the author had had access to different sources or if he had shown more critical acumen in the handling of his material.

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JOHN LAMPHEAR. *The Traditional History of the Jie of Uganda*. (Oxford Studies in African Affairs.) New York: Oxford University Press, 1976. Pp. x, 281. \$22.50.

John Lamphear's study of the Jie past is more than an outstanding contribution to the writing of East African history. With ingenuity and clarity, Lamphear has dealt with the myriad problems of reconstructing the past of a people without a written historical tradition, and without strong, centralized institutions.

Today, the Jie are a Central Paraniotic speaking people, numbering between 30-40,000 persons, living in northeastern Uganda. Lamphear spent sixteen months in 1969 and 1970 in Najie recording some two hundred interviews as well as doing both ethnographic and archeological fieldwork. This admirable monograph—well written, illustrated with maps, and indexed—has developed from that study.

In a stunning methodological chapter, Lamphear discusses the problems of reconstructing the

Jie past, giving particular attention to the handling of oral tradition. The major problem which Lamphear faced was that in such a segmentary social setting as Najie there was no "central tradition" to give coherence to the mosaics of the past retrieved from disparate tradition. Lamphear has turned this apparent handicap to his advantage, finding that the distinctive local and family traditions of narrow, yet detailed, character provide more verifiable and useful images of the past than the general traditions circulating among the Jie. He has produced something that might be called "Lamphear's law," that "group traditions often tend to become progressively less reliable, the larger the group."

Much the same approach is used by the author in building a chronology of the Jie past. With the Jie, Lamphear had no "king-list" such as many African historians rush to in attempting to erect a chronological scaffolding. Nor did Lamphear have much more than shallow genealogies with which to lace such a chronological frame. What the author did have in Najie was a patterned memory of generation-sets based on a concept that a "generation" is born together. All the sons of a man will be identified with the generation-set which follows the father's own generation-set. And Lamphear, by careful application of a variety of data, is able to reconstruct a sequence of eight generation-sets extending back from the 1960s. The problem for the historian is then to establish estimated dates for the inauguration of each generation-set in the past; that is, when the cycle of initiations to each newly conceived generation-set commenced. By a study of life histories in the late nineteenth and early twentieth centuries, Lamphear establishes an estimate of forty years for the interval between the inauguration of one generation-set and the inauguration of the next. His argument that this estimated interval was roughly the same in the more remote past is persuasive. This estimated interval becomes, then, the basis for estimating the crucial inauguration dates for each generation-set. In turn, events and individuals associated with the active periods of particular generation-sets in Jie tradition may be assigned estimated dates in the past.

The largest part of the body of the book is concerned with the movement of population in the region of what is now northeast Uganda and with the settlement of the Najie area from the end of the seventeenth century. Here the formation of the Jie territorial divisions found in the early twentieth century is reconstructed in detail. Lamphear makes a very important contribution to East African studies generally in discriminating between pastoral groups in the Najie region and what he calls "agricultural Paraniotic" peoples. The sig-

nificance of these agricultural groups, and their influence upon more remote groups, has not heretofore been recognized. The terminology which Lamphear creates and deploys for dealing with the layers upon layers of linguistic, economic, social, and cultural groups in this region has been, and will continue to be, especially useful to students of the region's past who have found the terminology up to now very imprecise and confusing.

The closing chapters of the book are given to a consideration of the relations of the Jie with their neighbors—extremely turbulent developments in the precolonial era and continuing into the colonial period. What comes through most clearly in the last section is the importance of the full adaptation to a differentiated microenvironment—which gave the Jie a strong base for expansion into neighboring regions from the nineteenth century on. This expansion continued into the colonial period and even into the early 1970s.

Lamphear has produced a remarkable book. Historians of Africa have much to discover in this volume, not least of which is the way in which the author has disclosed the cultural and economic orientations of historical groups from rather parochial family and local traditions. Lamphear sets a very high standard for historical studies oriented toward the migration and settlement of African people in the precolonial past.

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IVOR WILKS. *Asante in the Nineteenth Century: The Structure and Evolution of a Political Order.* (African Studies Series, number 13.) Cambridge: Cambridge University Press. 1975. Pp. xvii, 800. \$45.00.

Ivor Wilks' brilliance, incisiveness, and gifts as a meticulous historian have been aptly demonstrated in his study of the Akwamu Empire and in his earlier essays on Asante. His present work, *Asante in the Nineteenth Century*, is a scholarly, original, and massive study of Asante that is unlikely to be surpassed. It is a book that will stand with the works of Philip Curtin as the most significant among to emerge in the study of Africa during the last two decades. Despite the title of the book, it is a work that goes beyond nineteenth-century Asante history, and although the author modestly claims the book is not intended as a comprehensive history of the Asante nation, in fact, it is.

The book falls into four sections, with the first three chapters dealing with a spatial structure and demography of nineteenth-century Asante. The next five chapters give an account of Asante's conduct of provincial and foreign affairs. Chapters nine through eleven offer an analysis of the ruling dynasty, the structure of the decision-making

process, and the nature of the Asante executive. The final four chapters discuss political polarization and Asante in the nineteenth century, political disorder and conflict in Asante, the policy of modernization associated with the Asante Prince Owusu Ansa, and the problems posed by a rising merchant class.

Wilks has consulted a wide variety of sources with great imagination and ability, and those who are familiar with the works of Bowdich and Dupuis and nineteenth-century British documentary materials will admire his use of these sources. He has also extensively used oral traditions and archival material in Holland and Denmark, as well as the Basel Mission archives and the Methodist Mission archives in London.

Much of the criticism the book will meet will center on his use of terms such as "confederacy," "War and Peace Parties," "mercantilism," "bureaucracy," etc. The basis of criticism, however, will probably be that of interpretation rather than substance. Wilks' frequent use of Twi words in describing Asante institutions throughout the book is enlightening. But, given the fact that most of the words he employs are Asante, and that the book has a glossary of principal Asante terms, it is unfortunate he often lapsed into using Akuapem Twi in the second half of the book.

The publication of *Asante in the Nineteenth Century* is a major accomplishment for Wilks as well as for African studies. With the appearance of this book everything previously written on Asante pales in comparison.

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CRANFORD PRATT. *The Critical Phase in Tanzania, 1945-1968: Nyerere and the Emergence of a Socialist Strategy*. New York: Cambridge University Press. 1976. Pp. x, 309. \$19.95.

The unfolding drama of Tanzania's efforts to establish a socialist society in East Africa has had almost a cobra-like fascination for students of African politics and history. In a continent where political survival and respectability demand that even the most conservative and corrupt leadership group pay lip service to the tenets of socialism, pan-Africanism, and participatory democracy, Tanzania stands almost alone in its attempt to formulate a program of action to match its verbal commitment to revolutionary political, economic, and social development. Unlike other leaders, moreover, who base their hopes for development on the illusive miracle of "instant industrialization," President Julius Nyerere has opted for a program of *agrarian* socialism. This is in recogni-

tion of the fact that 90 per cent of Tanzania's sixteen million inhabitants will continue for some time to come to reap their daily sustenance from agriculture. Nyerere's bias against urban growth thus serves the humanitarian objective of meeting the needs of the majority as well as attempting to avoid the accelerating rate of urban unemployment which plagues Nigeria, Zaire, and other African states.

What makes the Tanzanian experiment in socialism so fascinating to scholars is that it is taking place in a relatively "open society." There are some political restrictions upon research, and the procedures for research clearance are cumbersome and time-consuming. Yet the best testimonial to the open character of Tanzanian society is the growing body of literature—much of it critical—representing a wide spectrum of ideological evaluation of the experiment in agrarian socialism. In the growing debate remarkably few scholars have attempted in a systematic fashion to address themselves to the question of why this radical departure from the mainstream should have occurred in one of the economically "high risk" countries; for, indeed, Tanzania is one of the poorest of those states that have been labelled the "fourth world," based upon their actual and potential prospects for economic growth. It is Cranford Pratt's distinct contribution to the growing debate that he sheds light on the historic circumstances and events which led the Tanzanian leadership to opt for a clean break with the conventional wisdom on development and launch the policy of *Ujamaa*, or Tanzanian socialism.

Pratt is a Canadian who was instrumental in the launching of what has become the University of Dar es Salaam. He had the distinct advantage as a social scientist of being significantly involved in Tanzanian affairs at the time when Julius Nyerere was reformulating his philosophies and strategies of development in an effort to create a just society. Although I would argue that the "critical phase" in Tanzania's development was actually far briefer a period than the 1945-68 dates Pratt refers to in the title of this work, he does provide a distinct service to the previously uninitiated reader by providing coverage of the last decade and a half of British colonial rule in Tanganyika—as Tanzania was then called. The period from the end of World War II and the achievement of self-government actually sets the stage for the continuation of the "dependency syndrome" against which Nyerere and the Tanzanian African National Union ultimately rebel. Pratt points out that it was accepted almost without question by Tanzanians and Britons alike in 1961 that the new nation would continue to rely upon British aid and investment, would require the services of a large cadre of ex-

patriates to manage both government and the economy, and would remain committed to the British institutional norms regarding the appropriate relationships among parliament, the cabinet, the civil service, political parties, and voluntary associations, such as the trade unions and cooperatives.

The actual "critical phase," which Pratt handles with a great deal of insight, begins in 1959, when the British government accepted Nyerere as head of the transitional government. This phase came to a focal point in 1967 when Nyerere and TANU committed the country to a path of socialist development in the Arusha Declaration. Pratt very skillfully juxtaposes the crucial events of this period against the backdrop of the evolution of Nyerere's political, social, and economic philosophies. The events which provide Nyerere with the need for dramatic action as well as with the base of popular support needed for the launching of a revolutionary program are well documented. These include the balance of payments crisis; the army mutiny of 1964; the confrontation with the students, the bureaucrats, and other elitist groups; the series of crises with the Western powers; and the continuing demands being made by the urban unemployed at the expense of the rural masses.

Even more skillfully handled is Pratt's analysis of the growing complexity of Nyerere's ideology, which evolves from an almost implicit faith in the moral worth of the individual into one of the more comprehensive restatements of socialist thought to meet the conditions of agrarian African society. It is a philosophy which regards human beings as both the primary resource in affecting change in economic growth as well as the principal end for which change is undertaken in the first place. True development, according to Nyerere, can only take place through self-reliance. Continued reliance on the money and services of outsiders deprives one of control over one's own destiny. Fearing, moreover, that rapid industrial change would create the conditions for perpetual class conflict, Julius Nyerere has demanded that the leaders and the masses alike undergo sacrifices in the achievement of a just society. Thus, the curbs on elitism are as essential to his program as the efforts to reorganize the rural masses into *ujamaa* villages. Both have as their objectives the rationalizing of production and the more equitable distribution of the fruits of common labor. Nyerere's rejection of the need for an industrial stage of development as well as his rejection of the inevitability of class conflict set Nyerere's brand of socialism apart from most versions of Marxian socialism. His emphasis on human love, moreover, are much more attributable to his Catholic training than to his latter-day exposure to Marx.

Essentially Pratt's history of the "critical phase" is a view from the top of the pyramid. Little effort was made in this volume to analyze the complexities of life at the grassroots which complicate the introduction of a radical ideology. The research of other scholars points out the lack of a uniform response on the part of the 120 ethnic groups included within Tanzanian society. One could also fault Pratt for his manifestly sympathetic treatment of Nyerere and his policies, but on this score he is far less guilty than many others of us whom Ali Mazrui has labelled "Tanzaphiliacs." One might also have been disappointed that a book that appeared in 1976 did not provide at least some discussion of current developments which would have placed the events and decisions of the earlier period in better perspective. But this would constitute the sin of criticizing an author for the book he did not intend to write. The scholarly world may yet have the benefit of that assessment from Pratt. In the meantime, we are indebted to him for his masterful presentation of the zero base against which the contemporary judgments must be made.

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JACK R. ROELKER. *Mathu of Kenya: A Political Study*. (Hoover Institution Publications, number 157.) Stanford: Hoover Institution Press. 1976. Pp. xviii, 202. \$8.95.

There are many books on the *uhuru* generation of Kenya nationalists. Yet, apart from Harry Thuku's autobiography and the odd academic paper, there is little about their predecessors, men whose political careers had ended by the time independence arrived. One turns with interest, then, to Jack R. Roelker's book on Eliud Mathu, the first African member of Kenya's Legislative Council.

Roelker has divided his book into three parts. The first describes Mathu's pre-LegCo years: his traditional African boyhood; his education in Kenya, South Africa, and England; his teaching career at the prestigious Alliance High School and as the principal of a primary school. The second section deals with Mathu's thirteen years in the Council from 1944 to 1957. The last two chapters contain a discussion of his defeats in the 1957 and 1958 elections and a brief review and analysis of his political life.

Roelker concentrates on Mathu's term in LegCo with the aim of assessing his contribution to Kenya's independence, and concludes that it was substantial. As evidence, he cites the formation of the Kenya African Union (KAU), the ending of

the *kipande* system, the increase from one to six nominated Africans in LegCo as well as African advances in education, cash crop production, and the civil service. More generally, Roelker believes that Mathu's "parliamentary ability was positive proof to the administration of the African's capability to effectively self-govern."

In addition to his part in the formation of the KAU, Mathu was instrumental in arranging Kenyatta's trip home. He also spoke up again and again for African civil rights in an atmosphere characterized at its best by what one of his European colleagues described to me as "benevolent irritation" and at its worst by outright hostility. Nevertheless, Roelker's claims are exaggerated.

These claims are no doubt the result of an understandable desire to show the reader how important Mathu was. It is also Mathu's fault. He is not only the supplier, but the interpreter of much of Roelker's data. Less moderate Africans attacked Mathu for his "half a loaf" approach, and he wants to justify his moderation. This was evident when I interviewed him during my own research.

There is another problem. the author should have placed Mathu's career within the larger context of Britain's postwar colonial policy of economic development and social welfare. To limit the analysis to Kenya is to tell only part of the story and, again, to overweight Mathu's accomplishment.

In the end, however, Roelker's book is useful. The story of Eliud Mathu adds a valuable dimension to the pre-Independence years in Kenya by helping us understand the difficulties that faced the African moderates, who have been overlooked too long.

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GARY WASSERMAN. *Politics of Decolonization: Kenya Europeans and the Land Issue, 1960-1965*. (African Studies Series, number 17.) New York: Cambridge University Press. 1976. Pp. ix, 225. \$22.50.

Gary Wasserman's *Politics of Decolonization* is an examination of the ways in which the essential features of the Kenya political economy, to a large extent dependent on the export contribution of a few thousand European planters, pastoralists, and mixed farmers in the White Highlands, were preserved during the transfer of power to African nationalists. His particular concern is the negotiation and implementation of the land resettlement schemes in the Highlands, whereby European mixed farms adjoining African reserves were purchased and broken up into small holdings for African peasants, and some larger farms scattered

throughout the Highlands were made available on very favorable terms for members of the new African political elite. In his examination of the negotiation of the land bargain in 1960-62 Wasserman sees the European "liberals" led by Michael Blundell as playing the key role, with the help, often behind the scenes, of Lord Delamere and the Kenya Farmers Union. Blundell's New Kenya Party enjoyed only minority support from the European settlers but it had sufficient African support to enter a coalition government with the Kenya African Democratic Union during the final negotiation of the land bargain. On the other hand the "conservatives" in Cavendish-Bentinck's Kenya Coalition maintained a diehard position of non-cooperation and demanded very much larger compensation. They were out-manuevered by the Blundellites, the Colonial Office, and the British Conservative government. In all of this the African nationalists, whether of KADU or KANU, stood on the sideline; they played no part in the initiation or negotiation of the bargain but went along with it as an essential prerequisite to independence.

Wasserman puts his analysis of the land schemes into a wider context. In terms of Kenya history it can be seen as part of the longer term process whereby European settlers, when balked in the political arena, fell back on gaining control of quasi-governmental boards. And, though independence came under African leadership and the Highlands no longer remained an exclusive white preserve, the bulk of the European pastoral runs and plantations remained intact. There was no mass exodus of European settlers, who continued to dominate the political economy. The resettlement of African peasants in parts of the Highlands along with land consolidation in the former reserves helped to create political stability. Many of the new ruling elite, who were rewarded with farms, were co-opted into the settlement. "In its aspirations, its techniques, its style, the 'independent' leadership remained *plus monarchiste que le roi*." Independence has been marked by a change of leadership from white to black, but the old colonial political economy has remained virtually unscathed.

This is a persuasive thesis, especially as it still seems to hold true for Kenya thirteen years after independence. But it is a little too neat and tidy. It exaggerates the influence and manipulative skills of the Kenya settlers and the British bureaucrats, and suggests that they were involved in a deep laid plot to perpetuate colonialism. They were, I suspect, more concerned to avert chaos and, so far as the British were concerned, to get out of Kenya as cheaply as possible. In any case Wasserman does not define colonialism; he simply takes it for

granted. Nevertheless this is a formidable study. The New Left will be proud of it.

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HEINRICH SCHOLLER and PAUL BRIETZKE. *Ethiopia: Revolution, Law and Politics*. (Ifi-Institut für Wirtschaftsforschung München, Afrika-Studien, number 92.) Munich: Weltforum Verlag, 1976. Pp. 216. DM 44.

This book is an attempt to provide an interpretative study of the Ethiopian revolution which began with the deposition of Haile Selassie on September 12, 1974. It also aims at assessing the impact of the military take-over upon the development of public law.

The first chapter describes the causes of the revolution in which the military at first seemed unprepared to assume government functions. The narrative, however, becomes confused on the issue of how the military junta took over power. On the conduct of international relations, the authors do not seem to foresee the diplomatic game of the military government in declaring Ethiopia a socialist regime in order to receive Russian support to prevent a Somali attack from the south and to restrain them from annexing Djibouti, depriving Ethiopia of the main railway connection with the sea.

In discussing the process of public law the authors point out that the revolution has been complete because the courts have recognized the legality of the new regime. Readers will find it difficult to accept the assessment because, although laws may have been manipulated to validate the military regime, the people of Ethiopia have not wholeheartedly accepted the new situation. The remainder of the book, on legal and constitutional events in Ethiopia under military rule, has a valid in-depth analysis and valuable criticisms that will be an asset to the specialist as well as to the well-informed general reader.

Overall, however, the book's aims are not completely fulfilled, since it tried to be a legal study and at the same time a historical narrative of the overthrow of the Ethiopian monarchy and subsequent events. The authors have used personal information and referred to secondary sources, as well as consulting government-published documents. Of great value are the volume's appendixes, including a table of a chronology of the revolution and various proclamations of the military government, especially those on land reform and the Draft Constitution.

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AXEL J. HALBACH. *Die Südafrikanischen Bantu-Homelands: Konzeption, Struktur, Entwicklungsperspektiven*. (Ifi-Institut für Wirtschaftsforschung München, Afrika-Studien, number 90.) Munich: Weltforum Verlag, 1976. Pp. vii, 252. DM 46.

When white South Africans established the Union of South Africa in 1910, they looked to the creation of a powerful dominion, wealthy and productive, that would one day comprise the whole of Southern Africa, and cast its shadow as well over East and Central Africa. In the economic sense, South Africans succeeded beyond their wildest dreams. In territorial terms, their hopes were disappointed. South Africa did not manage to absorb the three British High Commission territories; they subsequently became independent states. Southern Rhodesia, in 1922, voted against union with South Africa, disappointing the hopes of its founder for creating a new British counterweight to the Boers. In World War I, South African and British troops conquered German Southwest Africa. But South Africa failed to incorporate the territory into the Union of South Africa. Afrikaner nationalists, moreover, prepared to make some territorial concessions within South Africa itself. They accepted the creation of "Bantu homelands" as an alternative for sharing power with Africans on a national level. The "homelands" were poor and weak. Most outside observers, as well as many white South Africans themselves, therefore considered the new course as a device for setting up helpless puppet states in order to deceive public opinion.

Axel J. Halbach regards this as a dangerous oversimplification. His account, succinct, well-informed, and clearly written, provides a different interpretation. He outlines the policies that led to the creation of the homelands. He explains their economic, political, social, and demographic problems. He ends with a sober assessment of their future. As he sees it, the homelands have produced a new African elite, recognized by white South Africans in a manner inconceivable a generation ago. The new elite is determined to defend the interests of black Africans, not merely within the homelands, but also within the confines of "white" South Africa proper. The "unitary" concept of South Africa, once unchallenged, has been abandoned—at least in theory—by all white South African parties, including the Liberals and Progressives. The homelands at present face enormous problems of an economic and financial kind. They are not viable without massive aid from the South African government. Nevertheless, the Transkei budget exceeds that of several African states whose independence is internationally recognized, for instance Botswana (a former High Commission territory), Dahomey, Upper Volta, Niger, Rwanda, and Mauritania. The author might have added

that, for all its economic weaknesses, the Transkei has a higher per capita income than many relatively developed African countries like Togo, Nigeria, Sudan, and Tanzania. The homelands, like Mozambique and Lesotho for example, remain, however, linked to South Africa's economic fortunes, especially as labor exporters.

Their political future remains hard to predict. But ironically, the erstwhile puppets may well present a serious challenge to the future status quo in South Africa. They will certainly seek to extend their borders. They will make further demands on the South African treasury, demands that the South African taxpayer will deplore, especially at a time of rising inflation. According to the author, the Bantustan policy, as practiced at present, is bound to fail. Only a new partition of South Africa, entailing major white concessions, may still have a conceivable chance of success. Any such solution seems improbable at best. But history has a consistent record for confounding the prophets.

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ASIA AND THE EAST

JOSEPH NEEDHAM, with the collaboration of LU GWEI-DJEN. *Science and Civilisation in China*. Volume 5, *Chemistry and Chemical Technology*. Part 2: *Spagyrical Discovery and Invention: Magistries of Gold and Immortality*. New York: Cambridge University Press. 1974. Pp. xxxii, 510. \$35.00.

The work of Joseph Needham and his colleagues has more than once been called the greatest work of historic synthesis in this half of our century. Less often has it been noticed that it typifies in a remarkable way the operation of geometric progression in the growth of learning. As the first four volumes appeared, each was, on the average, double the size of the one that preceded it. Volume four had to be divided into three parts, and even so the third part is too ponderous to be read in bed. Length now having been standardized by policy, natural increase can assert its inexorable pressure only in the multiplication of fascicles. Alchemy, announced in a prospectus issued about fifteen years ago as one section of one part of volume five, is now materializing as four tomes, of which part two is the first. Part one, on military and textile technology, paper and ink manufacture, and printing, is largely still to be drafted by Tsuen-hsuei Tsien, Jung-pang Lo, and other contributors.

Part two consists of several preliminaries to the chronological survey to come in part three. The richness of this book (and its 165-page bibliography) will astonish readers whose idea of alchemy was formed by the scanty surveys of O. S. Johnson

(1928) and M. Chikashige (1936). A brief history of scholarship on alchemy precedes a section entitled "Concepts, Terminology, and Definitions," which sets out the understandings and assumptions on which the historical analysis rests. Alchemy is taken to mean the combination of "macrobiotics" and "aurifaction" (p. 9). The former term refers to "the belief that it is possible to prepare . . . drugs or elixirs (*tan*) which will prolong human life beyond old age . . . , rejuvenating the body and its spiritual parts so that the adept . . . can endure through centuries of longevity . . . , finally attaining the status of eternal life and arising with etherialised body as a true Immortal . . ." (p. 11). The latter coinage is defined as "the belief that it is possible to make gold . . . indistinguishable from, and as good as (if not better than), natural gold, from other quite different substances. . . ." Aurifaction was, Needham claims, distinguished in every civilization from "aurifiction," "the conscious imitation of gold . . . and other . . . substances . . . the proto-chemical artisan must be aware that his product would not stand up to the fundamental test of cupellation" (p. 10). The authors deduce from these last two definitions—which attempt to separate clearly the lore of the gold-faking craftsman from the notions of the gold-making philosopher—that alchemy began in China: ". . . it is clearly essential that alchemy should be distinguished from aurifiction and aurifaction alone; if so, the Hellenistic protochemists ought not to be called 'alchemists,' for there was little or no macrobiotics in their thinking" (p. 12). I will return to some problems raised by these definitions.

The same section then presents a scheme of ideas about the afterlife and the fates of souls in all the classical cultures, and places in this framework Chinese notions of immortality. These must be material, for the body was the indispensable "thread of a necklace" on which the three yang souls and seven yin souls were, so to speak, strung. Thus in China only immortality in the flesh—refined and incorruptible, to be sure—was conceivable (p. 92). An extended discussion of "liturgy and the origins of Chinese alchemy" turns out to be a monograph on the ingredients of incenses, with due attention to psychotropic substances and their possible contributions to religious experience. The section ends with a long table of chemical substances, ores, and minerals in the alchemical literature—with early Chinese and European names—and references to scholarship, a research tool made additionally useful by attention to changes in Chinese terminology over time. The many plant substances used by alchemists were drawn from the materia medica, and will no doubt receive similar treatment in one of the volumes on medicine.

A third section, entitled "The Metallurgical-chemical Background; Identifications of Alchemical Processes," is a remarkable systematic survey of various processes used in alchemy to produce imitations of gold and silver, both by alloy compositions and surface treatments. It is full of important discoveries and novel explanations. For instance, recent Chinese monographs have recognized the role of zinc and nickel in early metallurgy, but here the whole story of brass and paktong is set out in order for the first time.

The volume ends with a section called "The Physiological Background; Verifications of the Efficacy of Elixirs." Why did alchemists and their patrons ignore the danger of elixirs that we know often contained mercury, arsenic, and lead? Earlier publications have suggested that the exhilaration which accompanies advanced mercury poisoning intimated to the practitioner the onset of immortality, and that the ability of heavy metals to retard decay of his corpse convinced others (for the death and consequent incorruptibility of the mortal body were considered proofs of success). Here the physiological effects of a broad range of elixir ingredients are surveyed, greatly enriching our understanding. An important new argument appears to the effect that arsenic (and probably mercury) in the early stages of ingestion would have had aphrodisiac and tonic effects on chronically undernourished alchemists (especially those with mineral deficiencies), and that this "rejuvenation" might well have lured them insidiously toward fatal doses.

How adequate is the conceptual understructure of macrobiotics, aurifaction, and aurifiction? Some such set of working themes (and implied hypotheses) is indispensable to an initial survey—just as important chains of reasoning must depend on educated guesses about dates, since only a handful of the two hundred texts cited from the Taoist Patrology (*Tao tsang*) have been dated with certainty even to the nearest century. This is, after all, the first attempt to trace the main threads that run through the large, obscure, and, until a half century ago, practically inaccessible literature of alchemy. Repeatedly the authors emphasize that their findings are tentative. Their assumptions have made possible a coherent, original, and fascinating reconnaissance, with many detailed comparisons among the world's major alchemical traditions.

Needham's use of his three concepts leaves some doubt in my mind whether their value will hold up through the continued critical study that will form a secure understanding of the Great Work; they provide a most useful guide to certain important aspects of Chinese alchemy and at the same time obscure others. Furthermore, a critical look at the

literature—even at Needham's own selection of evidence—makes a considerable amount of qualification necessary.

Macrobiotics: It is true that the idea of an elixir as a pharmaceutical instrument of immortality appears early as one aspect of Chinese alchemy, but not in Alexandria. The emphasis of this book on the *product* of the Great Work is no doubt appropriate to the authors' view of alchemy as primarily a precursor of modern chemistry. It acknowledges but cannot do justice to the great importance in both cultures of a broader view, in which the alchemical *process* is seen as simultaneously the perfection (neither merely material nor merely spiritual) of a substance and of the alchemist.

This book acknowledges (p. 21) that the Alexandrians were "educated dilettanti who found in the operations of . . . artisans something that could be used for making models of the cosmic processes and cycles . . . in order to achieve a kind of salvation or release from the transience of uncomprehending mortal man." The Chinese alchemists tell us at great length that they were doing much the same thing and toward similar ends, as will appear in my contribution to volume five, part four. Nevertheless, in part two the definition of alchemy stresses differences, and dictates that the Alexandrian art was not "true" alchemy.

We are told that "by the end of the +3rd century the theoretical aspect of aurifaction changed, and under the influence of Hermetic doctrine could scarcely be distinguished from a soteriological quest of a redemptive nature" (p. 28). But there is room for considerable difference of opinion as to whether what the authors consider the outcome of a historic shift was not implicit in the earliest documents.

The assertion that the terminology of redemption in Hellenistic alchemy was "essentially metaphorical" (p. 73) could as easily be made about the Chinese tradition (see, for instance, the text in *Isis*, 67 [1976]: 525). That something is metaphorical does not mean that it is negligible, even in the history of science. The problems remain on both sides: What was the significance of the metaphors? What were the contexts of idea and belief that moved people to use them?

Aurifiction/aurifaction: Needham and Lu claim that the gold-faking of the artisans and the "gold"-making of the philosophers were "quite distinguishable . . . in every civilization." Indeed they are logically distinct, but historically they are not quite distinguishable anywhere. On the Alexandrian side, because early documents of aurifiction include procedures of dubious practical value and refer to philosophers, Needham and Lu warn us that "the people of the papyri were certainly not artisans in the ordinary sense" (p. 29). On the

Chinese side, the authors draw evidence for knowledge of cupellation from the writings of one alchemist after another (pp. 60, 207), who thus shared this crucial test of the artisan. It takes more to dispel the fuzziness of these distinctions than a statement that Ko Hung and others like him "were carrying out aurifaction rather than aurifiction because what they produced was artificial gold according to their own definitions" (p. 70). Alchemists had no choice about considering artificial what they themselves had made.

Similarly, the authors consider the *Pao tsang lun* (ca. 918) "nearer to the artisans than to the Taoist religious philosophers and alchemists" because its list of gold and silver varieties distinguishes real and false (p. 281). But the same gold list, with the same distinctions between genuine and artificial golds, appears a century earlier in a book Needham and his collaborators in volume five, part three describe as "a florilegium of alchemical writings" (p. 158)!

The distinction between aurifiction and aurifaction must also be approached sociologically, it is plausibly argued, for "there was a radical difference of social class between the artisan metalworkers and the dilettante philosophers" (p. 46). But Needham's approach to the role of Taoism remains remarkably unsociological. The term refers now to a high philosophy, now to a popular religion, now to a sentiment in everyone's mind. One never learns where the Taoism that is said to have shaped alchemy (e.g., p. 9) was located and how it operated in Chinese society.

The distinction between aurifiction and aurifaction is an original one, and as the last few paragraphs make clear, encourages reflection on previously unexamined issues. Nevertheless, the evidence does not persuade me that it is a clear distinction, or a necessary feature of any sound definition of alchemy. Nor do Needham and Lu measure the importance in the literature of the observations on which it is based. Important traditions within alchemy, which can be given only very limited attention in this preliminary survey, are not concerned with pseudo-gold, so that the issue of cupellation is of limited importance.

This conceptual undergirding, in sum, captures an important aspect in which early Chinese alchemy is different from analogous activities elsewhere. So much for the varieties; what distinguishes the species alchemy from the rest of human enterprise? I do not believe that any definition which confines its view to chemistry and biochemistry can adequately compass the character of the alchemical quest, any more than the point of alchemical liturgy can be conveyed by discussing incense ingredients, however fascinating. Needham is much more aware of the religious,

metaphysical, and moral dimensions of early science than were his predecessors. Once acknowledged, however, these dimensions often have little influence on his central characterizations, which depict early ideas and practices primarily as precursors of today's scientific thought and technique.

It is only the unprecedented standard of Needham's scholarship that demands such critical examination of his historiography. Despite my reservations about certain assumptions, this book marks a great improvement over earlier writing, in which there are few sophisticated ideas to examine. Future explorers of Chinese science will form their first overview of alchemy, as of so many other aspects of science and technology, from a vantage point on the shoulders of Needham and his colleagues at the East Asian History of Science Library.

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EDWARD L. FARMER, *Early Ming Government: The Evolution of Dual Capitals*. (Harvard East Asian Monographs, number 66.) Cambridge, Mass.: East Asian Research Center, Harvard University; distributed by Harvard University Press, 1976. Pp. xi, 271. \$15.00.

The founding emperor of the Ming dynasty, T'ai-tsu (r. 1368-98), after driving the Mongol overlords out of China in 1368, established the new dynasty's capital at his rebel base at Nanking in the populous, cultured, and prosperous south. In the early 1400s the third emperor, Ch'eng-tsu (r. 1402-24), gradually moved the Ming central government to Peking, his own power base in the relatively underpopulated and underdeveloped north. The transition was formally consummated in 1421, after which Nanking was relegated to the status of a largely symbolic secondary capital. Between 1368 and 1421 there had been some early indecision about Nanking's suitability as a permanent capital, some innovative arrangements to provide effective military leadership and logistical support for defense of the still volatile northern frontier, and a great deal of complicated planning and work in preparation for the eventual transfer of the capital, including, for example, the rebuilding of the Grand Canal.

This sequence of events, familiar to all students of Chinese history in its general outlines, is the subject of Edward Farmer's small book. He does not provide an overall study of early Ming government, as his main title suggests. Nor does he explore any one aspect of the Nanking-Peking transition in the analytical detail one normally expects of a research monograph. What he does is bring together information about many aspects of early

Ming history, policies, and administration that relate to the Nanking-Peking transition and help to explain the why and how of events, in topical essays linked together loosely by the contention that the transition brought about something new and important in China's institutional history, which Farmer conceptualizes as a "dual capital system."

I find Farmer's effort disappointing. To be sure, there was an interesting division of capital functions between Nanking and Peking for a decade or so before 1421; thereafter Nanking's role as auxiliary capital (which is not explored in any detail here) was somewhat different from the roles of secondary capitals in prior periods; and the choice of Peking as the permanent Ming capital had military and other consequences of some significance (which Farmer also touches on but lightly). I cannot, however, share his enthusiasm for the idea that the Nanking-Peking transition involved institutional changes of major importance, or his fascination with the concept of a dual capital system as a good way to explain the relationship between the two cities and to throw new light on early Ming events. His book nevertheless offers sound information about early Ming history from a new point of view, even if it is not organized into a persuasive overall thesis.

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TU WEI-MING. *Neo-Confucian Thought in Action: Wang Yang-ming's Youth (1472-1509)*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xvi, 222. \$10.00.

The philosopher and statesman Wang Yang-ming (1472-1529) revived Neo-Confucianism so successfully that he both commanded the respect of serious scholars and won a place as hero in sixteenth-century popular fiction. Wang's ideas stimulated the proliferation of lively, heterogeneous, and potentially rebellious philosophical schools in China; they also inspired scholars in Japan. Despite the fact that his influence was widespread both within China and abroad, accounts of Wang in English are few and sketchy. Tu Wei-ming's rich and probing biography of Wang's early years is therefore much needed.

Tu concentrates on how Wang drew on the available intellectual traditions to define sagehood for himself. Inspired by Erik Erikson's work on Martin Luther, Tu enriches his discussion of Wang's quest for sagehood by structuring the biography around Wang's spiritual crises. During one crisis, for example, Wang debated a choice between family commitment and Taoist with-

drawal, and during another he grappled with the powerful views of the twelfth-century Neo-Confucian philosopher, Chu Hsi. While always focusing on philosophical problems, Tu explores the ways in which personal experience shaped Wang's intellectual development. Particularly fascinating is the author's examination of how the history of Wang's family—which included a colorful hermitic grandfather and, so Wang believed, a famous fourth-century calligrapher—shaped Wang's aspirations and expectations. Here Tu imaginatively suggests that family traditions can challenge men to engage in the kind of intellectual dialogue that leads to creative philosophical thinking.

To determine what drove Wang to reinterpret Neo-Confucianism, Tu every few pages asks such questions as why Wang remained confident in the perfectibility of man even while scholar-officials suffered under the despotic court, why his interest in Taoism mounted on his wedding day, and what compelled Wang to make peace with his antagonist, Chu Hsi. Such questions reveal Tu's own involvement in the philosophical debate and in the process convince the reader that ideas have power and that Confucianism even now is, as Tu claims, "a living tradition."

Tu intentionally chose to write for the specialist and to concentrate on the philosophical and psychological dimensions of Wang's search for sagehood. Consequently he sketches in only briefly Wang's social and political environment. Although acknowledging the importance of the stifling examination system, the despotic government, and the democratization of education as influences on Wang's thought, Tu lets these factors recede into the background. Thus, while providing us with an exceptionally thoughtful treatment of Wang's early philosophical development, the author also whets our appetite for further exploration of how Wang's sensitivity to his society shaped the particular direction of his ideas and whether this sensitivity was also an important basis for Wang's greatness.

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LAWRENCE D. KESSLER. *K'ang-hsi and the Consolidation of Ch'ing Rule, 1661-1684*. Chicago: University of Chicago Press. 1976. Pp. xi, 251. \$22.00.

This volume deals with the early part of K'ang-hsi's reign (1661-1722), with special focus on his contribution to the "consolidation" of Ch'ing rule between 1669 and 1684. However, it investigates "only policies and developments" integral to the central theme of "consolidation" (p. x).

From the beginning, the early years of K'ang-

hsi's reign were attended by tensions generated by conflicts between Manchu tribal nativism and the ruler's imperial goal. After an introduction to the Manchu conquest prior to 1661, the book dwells on the Oboi regency (1661–69); in discussing Oboi's reactionary policy toward cultural assimilation, Lawrence D. Kessler's interpretations and conclusion "generally coincide" with Robert Oxnam's recent study of Oboi. Chapter three touches upon K'ang-hsi's childhood and early education during the Oboi regency; it also considers some of the most important figures surrounding the young emperor. A detailed description of K'ang-hsi's military strategy and accomplishments then traces the entire Manchu conquest through to its completion in 1684. Chapters five and six are crucial in providing evidence for the thesis: chapter five deals with "consolidation of bureaucratic control" and chapter six with K'ang-hsi's cultural policy toward Confucian learning and Chinese (mostly southern) literati. "As the exemplar and patron of the arts," Kessler asserts, K'ang-hsi "had bagged the scholar's heart" just as in the role of strategist he "had buried the soldier's hope" (p. 166).

The conclusion appears rather sketchy and unclear. If, for example, the Ch'ing regime was fully consolidated only "by the mid-eighteenth century" (p. 169) in that "ethnic barriers" had been fully eradicated—as Kessler quotes Nivision—then it is difficult to determine the degree to which K'ang-hsi contributed to that consolidation during his first fifteen years of actual rule. If one conceives of "consolidation" as a process consisting of two fundamental and complementary aspects—the long, arduous process of cultural assimilation on the one hand and concrete political and institutional innovations on the other—then it appears that K'ang-hsi's major contribution to Ch'ing rule during these early years was conquest, not consolidation. For it was only after Manchu conquest was completed in 1684 that K'ang-hsi and his successors began to make political and institutional innovations (the palace memorial system, the Grand Council and the system of imperial succession, etc.). It was also during the post-1684 years that Manchu tribal nativism finally gave way to the rising tide of sinicization. Concerning major sources, the author could have made more efficient use of the most common—and most important—primary source, the *shih-lu* (The Veritable Record), for some of the chapters (such as that on early education). Similarly, K'ang-hsi's *T'ing-hsun ko-yen* (Family Instructions), which contains valuable references to his early experiences (education, San-fan Rebellion, his grandmother's influence, etc.), is not even included in the bibliography.

Despite these limitations, this volume is certainly an interesting addition to a growing body of

modern scholarship on one of the most prominent imperial personalities in Chinese—and world—history.

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DANIEL L. OVERMYER. *Folk Buddhist Religion: Dissenting Sects in Late Traditional China*. (Harvard East Asian Series, number 83.) Cambridge, Mass.: Harvard University Press. 1976. Pp. xi, 295. \$12.50.

Sectarian societies in China have been used and abused, distorted, maligned, and confused for too long by too many. Daniel Overmyer has attempted to unravel the historiographical tangle and bring some clarity to the study of millenarian sectarian cults.

Our view of the sectarian religions has been obscured by the Confucian bias that these are political movements masquerading under the guise of religion. To official historians, they were pernicious heresies threatening popular morals and social stability; some modern historians have turned this view on its head, claiming that the sects were the outlets for the rage of oppressed masses. The problem with both views, as Overmyer points out, is that they do not take seriously the religious nature of these organizations. Why did they persist over decades or centuries of peace and stability? Why do the cultic aspects remain central? Why, if they were the channel of discontent, were only ten percent of rebellions linked to religious groups?

The sects were also the pariahs of the established religions. It is not surprising that Confucians were appalled by millenarian cults, which called for an eschatological transformation of a social structure which to Confucian minds was based on the highest moral truths. Buddhists and Taoists were if anything even more disturbed, for these unwashed and unlettered millenarians gave a bad name to their own doctrines and threatened their fragile detente with the Confucian establishment. Not only did the sectarians offend because of their folk beliefs and rampant syncretic attitudes, but they represented a different type of spirituality, the more personalized and emotive spirituality which characterizes sectarian forms of religion.

Overmyer asserts that sectarian religions can be properly understood in both their religious and political aspects by taking seriously their fundamental religious intention as millenarians. Their basic doctrines were a fusion of several strains of Chinese religiosity. The main ingredient was Buddhist: simple Amidist lay piety, coupled with a faith in Maitreya, the future Buddha, who would come with a new emperor to destroy the wicked

and establish a perfect world. Into this were fused elements from Taoism and folk religion (most importantly a belief in the Eternal Mother, creatrix and savior of mankind, who would send Maitreya to save her erring children).

This book is a landmark contribution to the study of late traditional religion. It makes the reader thirst for more—more information on doctrines and practices, more explanation of the thought content of popular scriptures, and more analysis of the social context and significance of these cults. It is to be hoped that this book will lead to further study of popular religion and culture in Ming and Ch'ing China. What social and political factors stimulated the growth of this particular form of religiosity? What impact did these religious groups have on social changes in their regions? How does the imagery and theology of these groups reflect the broader historical forces in late traditional China?

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IRWIN T. HYATT, JR. *Our Ordered Lives Confess: Three Nineteenth-Century American Missionaries in East Shantung*. (Harvard Studies in American-East Asian Relations, number 8.) Cambridge, Mass.: Harvard University Press. 1976. Pp. xv, 323. \$18.50.

Irwin T. Hyatt, Jr. analyzes the careers of three nineteenth-century American Protestant missionaries located in northeast China. They illustrate, he implies, the "diversity of Christian witness in non-Christian lands" and act as examples of how Americans adjusted to life in a foreign environment.

Tarleton Perry Crawford and Charlotte (Lottie) Diggs Moon were Southern Baptists—conservative, evangelical, and intolerant of the intellectual trappings of the modern world. Crawford's authoritarianism drove him into bitter dispute with his mission board and his missionary colleagues. His Chinese parishioners deserted him, and he died embittered. Lottie Moon developed women's work, bettering the condition of Chinese women who came to know her. While few Chinese women converted, Southern Baptists today consider her the heroine of their mission movement.

Calvin Mateer, a Presbyterian, expressed faith in human progress and the essential goodness and improbability of humankind. He achieved distinction as a leader in the development of Western education in China, as well as composing many texts dedicated to making the Chinese language easier to learn.

The goal of the three was to develop the means needed to impart their conception of Christianity

to the Chinese, and thus to bring about conversion. The endeavor took decades of time and massive self analysis. The effort, the pain of failure, and the triumph of success are graphically illustrated by Hyatt.

Unfortunately, Hyatt's intriguing study must be faulted because of his limited use of Chinese sources. We are overwhelmed with data on how the three missionaries perceived themselves, and how fellow missionaries perceived them, but we know very little about the Chinese side of the story. Except for the writings of Mateer's students, we do not know what the Chinese thought. The missionaries tell us what they believed the Chinese were feeling, but we cannot confirm these perceptions through Chinese sources.

Hyatt also must be faulted for his tendency to wander. His digression describing the commercialization of Lottie Moon by Southern Baptists today seems superfluous. Further, the section on her begins with a lengthy discussion of the women's movement in north China. While this material is valuable in itself, one is left wondering whether the author will ever get to his primary subject. Hyatt might better have centered his account upon Lottie Moon, allowing the other missionary women to flow around her as occasion required.

While this book deserves attention within the literature of the missionary movement in China, the author has not made the most of his opportunity. His drifting style, as well as his limited use of Chinese sources, have caused him to miss the chance of writing an excellent and definitive study.

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JOSEPH W. ESHERICK. *Reform and Revolution in China: The 1911 Revolution in Hunan and Hubei*. (Michigan Studies on China.) Berkeley and Los Angeles: University of California Press, for the Center for Chinese Studies of the University of Michigan. 1976. Pp. xi, 324. \$15.00.

For nearly twenty years the study of the Chinese revolution of 1911 has itself been undergoing a revolution. On no other topic has controversy been more lively or research more fruitful. In all of this discussion a particularly valuable contribution has been made by local histories, studies of individual provinces or other well-defined regions of China. Though not all published, they now cover nearly every major part of the country. Since the two provinces he deals with include the one that touched off the revolution, Joseph W. Esherick's book adds a crucial piece to the jigsaw puzzle. In this regard he nicely complements Charlton M. Lewis' new book on Hunan, those by Mary

Backus Rankin on Shanghai and Chekiang and by Edward J. M. Rhoads on Kwangtung, and a number of other studies still in dissertation state. As a descriptive work of local history, Esherick's book fully belongs in this excellent company. It is very well researched and clearly written. Although there are occasional lapses, such as the use of data from the 1930s to describe conditions in 1906 (pp. 59–60), he handles a wide variety of sources competently. He supplies rich detail and fresh perspectives on events that other Western scholars have treated, such as the T'ang Ts'ai-ch'ang, P'ing-Liu-Li, and Wuchang uprisings, and on some that they have not, such as the Changsha riot of 1910. Esherick also pays more serious attention than anyone yet has to the effects of the Ch'ing reforms on the "masses," and while his view that the masses reacted wholly negatively to the reforms is not altogether persuasive—for one thing it leans heavily on foreigners' reports of mass anger and violence, and my own research in French archives suggests to me that foreigners were so ignorant of and frightened by the Chinese masses that their estimates were frequently exaggerated—it is at least a reasonable hypothesis and should be tested in other parts of China. There was certainly a great deal of popular discontent, but the full extent of it, as compared with non-reform periods and places, remains to be measured, and the reasons for it remain to be determined.

One might be more inclined to accept Esherick's hypotheses if his analytical framework were stronger, his conceptualization more rigorous, his biases and preferences more controlled, and his judgments more solidly grounded in historical perspective. His sympathy for the masses and his distaste for the elite are so pronounced that the reader wonders if either has been accurately portrayed. More important, Esherick's conceptual framework is limited to the categories "elite" and "mass," and these two shaky poles will not quite support the elaborate superstructure he builds on them. Perhaps Esherick cannot be blamed for the use of the term "masses," which ought to be seriously questioned, especially when applied to as numerous and diverse a population as China's. Indeed, his analysis has little to do with the masses, for the eighty percent of China's population that lived in the countryside enter the book only marginally, and, as Esherick confesses, data on them are scarce. The non-elite people he discusses are mostly what he considers a "lumpenproletariat," a Western category of the very sort that Esherick attacks other scholars for using and which he employs without the slightest attempt to distinguish it from such famous and very different Western usages as Marx's in "The Class Struggles in France." The term "elite," on which much of

the analysis in the book rests, is also used with some lack of precision. It is too blunt an instrument, too gross a category, for the task Esherick has assigned to it.

Esherick treats the elite as a class, but this overlooks how much it had changed in the years since it banded together behind the reactionary policies of the Empress Dowager in 1898. By 1912 it was much less homogeneous. There was a new military elite that was very different from the old, and a civil elite whose power and authority stemmed from diverse sources. Within the Chinese elite, in brief, there were many new conflicting interests and interest groups, including new structures such as political parties and chambers of commerce. This is a major reason for the chaos of the Republican era. Esherick sees the revolution as "a victory for the increasingly Westernized urban reformist elite" which saw in the revolution its "best chance to preserve the structure and stability of the existing society"; all the Chinese peasantry got was "a new regime in which gentry and officials were allied even more closely in defense of the class interests of the elite." He even goes so far as to speak of the revolution as having been under "tight elite control." All of this seems to me to exaggerate the unity of the elite and to ignore the many social and political changes that occurred in China between 1900 and 1912. To describe it as Westernized, urban, and reformist is already to describe it as radically changed. We shall have to wait for detailed studies of the 1912–49 period to resolve this issue, but in the meantime it seems to me that class analysis is inadequate for treating the fragmentation of a class.

Esherick's focus, in brief, has drawn him into several contradictions. He declares that he concentrates on "social structure" and virtually dismisses values, "psychology and mood." And yet on the one hand he falls back on "the mood of malaise" and "general transformation of the political climate" in 1911, and on the other hand he ignores crucial changes concerning youth and women. What he means by social structure is only class, but in China no features of social structure were more important than lineage and the domination of women by men and of the young by the old. To analyze social revolution in China with no reference to the feminist and youth movements and their effects on family and kinship relations, and then to draw the sweeping conclusion that the revolution of 1911 was "socially regressive," is to caricature social analysis. It is equally misleading to speak of the revolution as "politically progressive" in some aspects, without explaining what those aspects are and what the standards are for progressive and regressive. There is a hint toward the end of the book, when Esherick agrees with

Kuomintang historians that their party carried on one trend of the revolution but terms it "the trend towards rule by a Westernizing urban elite." This is a promising idea, but it suggests that his categories of progressive and regressive are moral and partisan rather than scholarly, and it brings out clearly how artificial is the line between the social and the political.

Esherick's idea of radicalism is also conceptually weak, for it excludes any new political behavior or structures that are "recognized as legitimate by the authorities of the state." This rules out such measures as the abolition of the examination system in 1905-06, which knocked the props from under the traditional social and political hierarchy; in the full perspective of Chinese history this must be considered a radical step. (If Esherick's concept of radicalism were applied to the People's Republic of China it would remove from the realm of the radical such measures as collectivization, the Great Leap Forward and the introduction of communes, and the Great Proletarian Cultural Revolution.) Is it really impossible for authorities to sanction radical policies? And yet after defining radical in this way, Esherick complains that in 1911-13 there was "a consistent denial of radical alternatives within the revolutionary governments," which seems to suggest something different.

Finally, Esherick raises the question of the role of "exiles" in the revolution of 1911. One of the major merits of recent studies is that they have turned away from what had been a heavy over-emphasis on the contribution made to the revolution by overseas Chinese businessmen and students who went abroad to study. To this healthy new stress on people and events in China rather than outside, Esherick now adds a far-reaching generalization: "Though exiled revolutionary conspirators may capitalize on revolutionary situations, they do not fundamentally cause revolution. The causes of any revolution must be sought within the country in question." No one asserts that the causes of the Chinese revolution should be sought primarily outside of China, but Esherick's formula may swing the pendulum of scholarship a bit too far. Indeed, since he holds that this was a revolution that was tightly controlled by a Westernizing elite, Esherick himself might well look for some important causes, at least indirect or secondary ones, outside of China. Historical causation is too old and tangled a thicket to enter here, but it seems clear to me that revolutionary exiles have been known to do more than capitalize on revolutionary situations. They help to create them. Lenin and others did, and so did many Chinese. Esherick tells of many students who returned from Japan to play key roles, and he also insists that a

revolutionary situation developed very rapidly in 1911. Much remains to be said about the relationship between exiles and events in China. It may even turn out that 1911 was as limited a revolution as it was partly because exiles who were out of touch with their own country were so prominent in it.

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C. MARTIN WILBUR. *Sun Yat-sen: Frustrated Patriot*. (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1976. Pp. x, 413. \$16.50.

C. Martin Wilbur has again produced another fine book, demonstrating the depth and breadth of his interest in Chinese history. This time he deals with an important but not yet fully understood political personality of modern China: Sun Yat-sen. He judiciously traces the influences shaping Sun's attitudes, and underscores the frustrations of his revolutionary career during the years 1895 to 1925. Two important aspects—often neglected if not ignored—stand out: Sun's endless fund-raising efforts among overseas Chinese and foreign sympathizers ranging from officials to entrepreneurs, and his perennial failures in acquiring aid from Japanese, French, British, German, American, and Russian leaders.

The first four chapters are devoted to the fund-raising and its failures (along with the influences shaping Sun's personality and his career). The other five chapters deal largely with Sun's attempts to get support from Soviet Russia. The recurring theme is that of Sun's search for support outside China to improve China's lot as well as his own. According to Wilbur, "on the one hand, [Sun was] planning for his country's welfare through economic and political reform and on the other, he was searching for shortcut methods to overthrow those rulers who stood in his way" (p. 99). In examining Sun's dealings with Russia, the author argues persuasively that, as always, Sun attempted to use Russia for China's benefit just as the Russians tried to take advantage of him for the promotion of their revolutionary cause in China.

Wilbur concludes that, despite his long fund-raising efforts, Sun failed to strengthen China politically and economically, or to weaken his political rivals at home. Wilbur attributes Sun's failures to the vast disparity between his overwhelming desire to make China rich and strong and his equally compulsive ambition to be the leader of his country. He also attributes Sun's frustrations to domestic and international realities which were beyond his control.

The persuasive part of this book lies in its comprehensive discussion of Sun's attempts to acquire support from Soviet Russia in 1922-25. It represents a significant breakthrough in the study of Sino-Soviet relations in general and Sun's attitudes toward Russia in particular: for the first time considerable use has been made of Russian materials in treating these subjects. Thus Wilbur, a Chinese historian by training, should be commended for his efforts. He also successfully incorporated the fruits of recent research on Sun's attempts to get support from British, Japanese, French, and American backers.

Although I have no serious criticisms about the book, I find its title somewhat misleading. More than half of the book deals with Sun's relations with Russia, and yet there is no such inkling in the title, not even in the subtitle. A misleading title may also dampen interest in the results of current research on Sun's relations with the other major foreign powers such as Britain, France, and America.

Wilbur's contributions will be valued by sinologists, and one hopes that he will continue his research, which certainly has benefited his fellow historians.

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TIEN-WEI WU: *The Sian Incident: A Pivotal Point in Modern Chinese History*. (Michigan Papers in Chinese Studies, number 26.) Ann Arbor: Center for Chinese Studies, University of Michigan, 1976. Pp. xiv, 285. \$4.00.

The Sian Incident remains one of the most dramatic episodes of modern Chinese history. Until now major English language accounts were those by James Bertram and Madame Chiang, the former a journalist and the latter the Generalissimo's wife. Now we have Tien-wei Wu's scholarly study, based on a careful reading of written materials, plus interviews with key participants.

Wu's book is particularly useful for its thorough treatment of the background to the Incident. It clearly depicts the sense of shame and lost honor which characterized the Young Marshall Chang Hsueh-liang and his Tungpei Army. They were quite susceptible to Chinese Communist arguments that Chinese should fight Japanese and not each other. Moreover, soon after arriving in the Northwest the Tungpei Army was routed in its engagements against the Red Army. Thus, Chang knew that exterminating the Communists would be costly. When the Central Government refused to help Chang rebuild his lost divisions, suspicions grew that Chiang Kai-shek wanted to "use one stone for two birds." This belief was certainly held

by the warlord of Shensi, Yang Hu-ch'eng, who first suggested the kidnap idea to Chang Hsueh-liang and collaborated in its execution. After talking with Chou En-lai, whose sincerity impressed him greatly, the Young Marshall reached the conclusion that a United Front was needed to fight the Japanese. Yet Chiang Kai-shek refused to alter his policy of first eliminating the Communists. He reiterated this theme during his first trip to Sian, in October 1936; he arrested seven leaders of the National Salvation Association in November; and he ordered his troops to fire on student demonstrators in Sian on December 9, 1936. Chang Hsueh-liang was forced to conclude that a *ping-chien* (remonstration with military force) was unavoidable.

Wu points out that the direct role of the Chinese Communists before the Incident was quite minimal. But afterward the CCP and especially Chou En-lai played a crucial role in securing Chiang's release. The Young Marshall was willing to release Chiang upon verbal assurances that the government would begin to fight the Japanese, but Yang Hu-ch'eng understandably wanted some written guarantee of his own safety. It was Chou En-lai who persuaded Yang to drop this demand and agree to Chiang's release. The Young Marshall voluntarily accompanied Chiang Kai-shek back to Nanking, publicly expressing remorse but privately expecting lenient treatment. To his surprise he was court martialed and placed under house arrest, where he has remained to this very day.

This careful, thorough study will be welcomed by scholars of Republican China. I would only quarrel with Wu's subtitle, which labels the Sian Incident as "pivotal." Chiang emerged from it with unequalled prestige, but he failed to use it effectively. Some Nationalist historians claim the Incident was at the root of the eventual Communist victory, but this seems far-fetched. No real resistance to Japan commenced. The Incident is a fascinating topic, and Wu does it justice, but it is hard for me to see it as pivotal.

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PHILIP WEST: *Yenching University and Sino-Western Relations, 1916-1952*. (Harvard East Asian Series, number 85.) Cambridge, Mass.: Harvard University Press, 1976. Pp. xi, 327. \$16.00.

Yenching University was founded in China in 1916 by American missionaries; matured in the environment of Chiang Kai-shek's Nationalist government; and finally succumbed in 1952, a victim of the clash between the Communist revolution and

America's East Asian policy. Throughout its existence, therefore, Yenching's development was determined by events and attitudes in both China and the United States. To describe this development, Philip West has written what he appropriately terms an "intercultural history."

In contrast to the fiery evangelical fundamentalists who had dominated the Protestant missions to China in the nineteenth century, the founders of Yenching were missionaries imbued with a liberal theology which stressed the humanity of Jesus rather than the wrath of God, good works rather than individual salvation, and cultural relativism rather than the superiority of the Christian West. John L. Stuart, dominant personality in the university and president from 1916 to 1946, for example, sought to formulate an "exportable Christianity" that would be "accepted alike by scholars and patriots and toiling masses, re-interpreted in Chinese terms and revivifying Chinese tradition" (p. 39). Yenching leaders thus sought not to challenge and overcome Chinese ways, because, as Stuart remarked in a manner rare among missionaries, "The Church has not very much to add to the moral or even to the philosophic ideas of the Chinese" (p. 42). They did believe, however, that Christianity would impart a "vital energy" to the Chinese, attaining thereby not just the personal salvation of individual Chinese but the strengthening of the Chinese nation.

West stresses that the Yenching leaders, with their liberal interpretation of the Christian message, attempted to promote rather than obstruct the goals of Chinese nationalism. They therefore readily complied when the Chinese government in 1925 proscribed religious instruction, and later when the new Nationalist government ordered compulsory teaching of the Kuomintang's ideology. As part of this sinification of the university, too, Chinese were progressively appointed to the faculty to replace Westerners. The original evangelistic purposes of the university were therefore entirely overshadowed by the attempt to create a truly *Chinese* university with highest academic credentials.

The crux of West's story is that, despite their willingness to accommodate the university to the prevailing social and political currents, Yenching's administrators and faculty members could not keep pace with the growth of China's militant nationalism. Indeed, the last three chapters of the book are devoted largely to the radicalization of the students and to the faculty's ineffectual attempts to cope with the increasingly revolutionary environment. Three years after the Communist victory, Yenching was shut down when Chinese resentment against the United States peaked during the Korean War.

This is not a work of major importance, for the subject, Yenching University, was itself of limited significance. Yet West has written a work characterized above all by finely drawn judgments. Imperialism, Christian missionaries, and Chinese intellectuals are etched in subtle shadings that enrich our comprehension of those complex and often crudely treated subjects. Students of modern China, of missionaries, and of Sino-American relations will all derive fresh insights from this fine monograph.

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DELIA DAVIN. *Woman-Work: Women and the Party in Revolutionary China*. New York: Oxford University Press. 1976. Pp. x, 244. \$15.00.

This book marks the "coming of age" of women's studies in the field of modern China. Building on the recent proliferation of excellent articles, Delia Davin has written the first monographic study of the transformation of women's lives in the People's Republic. It is a credit to the author that she has avoided the tempting but impossible task of analyzing *all* aspects of China's "women's revolution" and chose instead to focus on the manageable and crucial theme of "woman-work." Although the title at first seems awkward, it captures effectively the different connotations of this slogan for Communist cadres and for women affected by party policies. Throughout the book, Davin explores changing patterns of female labor participation and thus reintegrates the question of "women's liberation" into a broader study of Chinese economy and society. In this way, she has transcended the unselfconscious projection of contemporary Western feminist concerns onto the course of Chinese revolution—a problem which has marred otherwise fine studies on Chinese women published recently.

The organization of the book is rather confusing, leaving the reader the task of untangling the major themes. A history of the women's movement between 1930–60 is scattered in the first three chapters: 1) "Women in the Qiangxi Soviet and the Liberated Areas," 2) "Women's Organizations" and 3) "Marriage and the Family." A much more thorough and explicit analysis is presented in the last two comparative chapters: "Women in the Countryside" and "Women in the Towns." Two related Marxist assumptions are discussed in various ways in all the chapters: a.) that class transcends sex, and b.) that women will be free only when fully involved in economic production. Davin proceeds to raise skillfully several inescapable contradictions that have resulted from the Chinese

Communist Party's attempts to translate these assumptions into viable social policy. The most important problem arises from the fact that Communist policies have been addressed to two disparate and sometimes conflicting constituencies: poor peasants and women. Consequently, the book offers a thorough discussion of the social tension and political upheaval that have accompanied the simultaneity of land reform, and marriage laws in three periods: the Qiangxi Soviet, Yen-an, and the early 1950s. Even when the party sought to address itself primarily to women's needs, it faces contradictory pressures between the political advantages of female labor participation and the economic costs of replacing unpaid household labor with collectivized facilities. Underlying all the fluctuations of "woman-work" in the period 1930-60 is the tension between principle and expediency—a problem which Davin describes well from the point of view of both the party and women.

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CHARLES B. BURDICK. *The Japanese Siege of Tsingtau: World War I in Asia*. Hamden, Conn.: Archon Books. 1976. Pp. ix, 274. \$15.00.

The siege is a classic subject of military history. It offers the historian unparalleled opportunities to probe the character of men at war, and it requires exacting attention to detail. It also demands that the historian relate such detail to the broader flow of human events. Readers will use these criteria to judge this history of the only major land battle of World War I in East Asia, the 1914 fight between Japan and Germany for possession of Tsingtau.

Charles B. Burdick approaches his subject from the German point of view. Skillfully exploiting German archival sources, memoirs, and interviews, he shows that the fight was hopeless from the start. Burdick makes the feelings of German defenders and the frustrations of Britons participating in the attack come alive. The same cannot be said for his treatment of the Japanese. Because Burdick has not looked far beyond the 1916 official Japanese history, they appear as little more than ungentlemanly, mechanized yellow ants.

The book abounds in detail. Burdick uses it to elucidate the tactical significance of the siege: Tsingtau marked the first air-to-air combat in history, demonstrated the difficulties of bombing capital ships, and revealed the devastating effects of long-range artillery. But the manner in which detail is presented often confuses. Relative strengths of attackers and defenders are not clearly spelled out; an appendix offering biographical detail on major figures is missing. None of the three maps provided pinpoints the initial landing place of the

Japanese or indicates the attackers' positions on successive days. Most importantly, Burdick, cursed by the problem of writing in English about a battle between Germans and Japanese in China, is woefully inconsistent in spelling personal and place names. This lapse is certain to infuriate East Asian experts and puzzle the general reader.

Burdick is least convincing when he tries to put the battle in historical perspective. He says nothing of the bitter debate over entering World War I that wracked Tokyo. There is no mention of inter-service differences over Japan's continental policies. Nothing is said of the battle's impact on Tokyo's negotiating position at the Paris Peace Conference of 1919. Yet Burdick asserts that Tsingtau was "more historically significant than the savage battles in the trenches of France" (p. 194). Military historians may dispute this statement on technical grounds. Diplomatic historians will find it difficult to reconcile with accepted interpretations of the international political consequences of the Russo-Japanese War a decade before Tsingtau.

Burdick gives us more detail about that battle than Schrecker, Chi, Lowe, or Nish. But historians who hope to understand its broader significance had best look to these and other sources.

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BERNARD S. COHN. *India: The Social Anthropology of a Civilization*. (Anthropology of Modern Societies Series.) Englewood Cliffs, N.J.: Prentice-Hall. 1971. Pp. xv, 164. Cloth \$6.95, paper \$2.95.

MILTON SINGER. *When a Great Tradition Modernizes: An Anthropological Approach to Indian Civilization*. Foreword by M. N. SRINIVAS. New York: Praeger. 1972. Pp. xviii, 430. \$15.00.

Both works are written by anthropologists with an interest in history, marking a welcome and growing trend in anthropology which combines a synchronic with a diachronic dimension in the comparative study of societies. Bernard S. Cohn gives a concise introduction to civilization, society, and history in India, and Milton Singer develops a comprehensive approach to structure and change in Indian society in a series of separate but complementary studies brought together in one large volume.

Cohn is that rare phenomenon in social science today—the historian and social anthropologist in the same person. His studies of landownership and rural social structure, politics, census and society, and elites and empire in British India—in addition to his ethnographic studies of north India—exhibit an impressive and fruitful combination of the two disciplines. In this volume he displays the same

skills by providing a brief but succinct account of the interplay between social systems and events in the recent history of India. Cohn notes the local civilizing processes as well as those imposed from the outside on an indigeneous civilization in the making of the multi-levelled and extremely complex Indian social structure. As an indication of the scale and difficulty of the problems involved, almost a third of the work is devoted to cultural, historical, and linguistic regions. The rest of the study relates social and cultural structure, caste, urbanization, and village organization, to India's history.

Singer's large book is the result of a lifetime spent professionally studying India. It is a considerable achievement which promises, in the maturity of its approach, more singular and imposing contributions from the author. Starting with Redfield's concepts of civilization and great tradition, Singer welds them to Weberian sociology in an Indian context, presenting a number of studies that must be treated as a single approach rather than treatments of *bhakti*, cultural performances, joint family, and industrialization in Madras, south India.

Several of the essays have been published before but the three longest and most significant studies appear here for the first time. "The Social Organization of Sanskrit Hinduism"; "Urbanization and Cultural Change"; "Industrial Leadership, the Hindu Ethic, and the Spirit of Socialism"; together with the useful introductions that connect the various sections of the book exhibit Singer's own development in his "more than a passage to India" as well as the increasing complexity and inclusiveness of his analysis. Building on and adding to Redfield's and Srinivas' concepts, Singer embraces culture, values, and modernization in relation to Hinduism and society, and concludes with a well argued critique of Weber and neo-Weberian accounts of tradition and modernity. Hinduism no longer appears to be an impediment to entrepreneurial processes. Rather, the latter are to be understood in relation to cultural performances, devotionalism, the social organization of tradition, urbanization, and the transmission of great tradition. These cumulatively effect a series of transformations in Indian society and culture which both keep it Indian and at the same time make it modern.

Without detracting from the achievement of Singer's work, we may note that his approach lends itself almost inevitably to concentrating on the contents of social processes in an urban setting, on the compartments social groups occupy in these processes, and on the channels of communication between these compartments. Alternative, and in some ways complementary, approaches would

concentrate on the relationships among the elements of the social and cultural system.

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ELIZABETH WHITCOMBE. *Agrarian Conditions in Northern India*. Volume 1, *The United Provinces under British Rule, 1860-1900*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xxviii, 330. \$12.00.

In the last two decades, agrarian conditions in the United Provinces of Agra and Oudh have been a major preoccupation of historical research on the Indian subcontinent. Thanks to the works of Sulekh Gupta, Walter Neale, Imtiaz Hussain, and Asiya Siddiqi, we know more about the workings of colonial agrarian policies in this part of the Indian empire than in any other province. Elizabeth Whitcombe's study marks a departure from this most respectable tradition in that it offers a very circumstantial account of not merely what was intended, but what was actually done and how it affected the lives of men in the concrete context of a stratified society and a delicately balanced ecology.

The utilitarian euphoria of earlier decades persists in this period as a stubborn hangover. The ideals of peace, public works, and "scientific" revenue administration continue, despite the evident havoc wrought on the poorer and less privileged sections of agrarian society. The extension of canal irrigation and the support of market development through investment in roads and railways produced the desired results: the arable was extended and the production of "valuable" crops increased. But the output of food crops, so far as one can see—there is no statistical evidence to prove this point beyond doubt—lagged behind the increase in population. The bulk of the agrarian society, small cultivators or landless peasants without the means to benefit from the innovations and thus augment their money incomes, suffered as a result. Contraction of waste lands, increased salinity through canal construction, and other negative consequences of "improvements," impinged on their meager livelihood. Lord Mayo's grandiose plans for a Department of Agriculture to improve and develop the agricultural resources of the country produced pitiful results. Its subsidiary in the North-Western Provinces had a total expenditure of Rs. 101,400 in one year when the revenue receipts stood at nearly 42.6 million rupees; out of the former amount Rs. 62,000 went into salaries. The *takavi* loans, 1/111th part of the land revenue in 1868-69, later came down to 1/900th of the total. The absolute amounts in question also declined steadily reaching the "contemptible sum" of Rs. 16,523.

Whitcombe does not offer her chilling evidence as a shrill polemic against imperialist exploitation. But her study does bring out the ultimate predicament of colonial rule: the contradiction between its very existence and the general welfare of the subject people, the high-minded administrator's sincere efforts notwithstanding.

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P. G. ROBB. *The Government of India and Reform: Policies towards Politics and the Constitution, 1916-1921*. (London Oriental Series, number 32.) New York: Oxford University Press. 1976. Pp. 379. \$23.00.

P. G. Robb's scholarly and well-written work (originally a London School of Oriental and African Studies thesis) is an important revision of commonly held views on Indian governmental responses to nationalist agitation in Lord Chelmsford's era. Robb focuses particularly upon Chelmsford's personal contribution: despite his apparent detachment and lack of forceful leadership, above all when contrasted to his predecessor Hardinge, he was nevertheless determined to guide India along the road to greater self-government. The Montagu-Chelmsford reforms of 1919, often attributed primarily to Montagu, were at least half Chelmsford's responsibility. Ironically, Chelmsford departed India unmourned, having satisfied neither the hard-liners, who would ruthlessly repress agitation, nor the nationalists, whose reception of progressive measures was inevitably colored by the tragedy of Amritsar.

Robb's argument is well supported in the case of the Viceroy himself. His moderation toward the more important Indian leaders, his insistence upon greater Indian representation in Imperial councils, and his support of arms restrictions were all steps taken in conjunction with the constitutional changes toward the achievement of the goal of eventual home rule. None of these measures, Robb argues, was taken either to appease the nationalists in general or to divide the moderates from the extremists, though they may have had that fortuitous effect. He does not claim his interpretation to be revolutionary, but he does provide a detailed presentation of Chelmsford's contributions.

That Chelmsford's policy was successful is more arguable. Delicate and subtle distinctions between tactical noninterference with nationally known figures such as Gandhi and repression of local, less-restrained leaders were hard to grasp—or to accept—at the provincial level, as bloodshed in the Punjab so thoroughly proved. Robb may be correct in stating that if the government had opted

for thorough repression, "then the Indian response might also have been different, and Chelmsford's legacy might have been a growing commitment to autocracy in the face of nationalist violence" (p. 277). Perhaps so, but in the end, Chelmsford—like Hardinge before him who had also made many conciliatory suggestions—was still responding to the question, "How should India be held?" and not "When shall India be independent?" Robb rightly contrasts Curzon's belief in permanent autocracy with Chelmsford's conciliation based upon faith in the eventual transfer of powers, but it is still useful to stress the similarities in the common Imperial framework and "the ultimate incompatibility of bureaucracy and self-determination" (p. 219).

This is not to criticize Robb's general conclusions or the substantial value of his work. He has mastered a mass of archival material, and although much of the ground is familiar—Amritsar, Khilafat movement, Rowlatt *satyagraha*—Robb is careful not to duplicate the work of Judith Brown, Ravinder Kumar, and others whose studies this work usefully supplements. Inexplicably, sixty pages of notes and references, many containing important information, are confined to the back of the book—a considerable nuisance for Robb's inevitably scholarly audience.

BRITON C. BUSCH
Colgate University

V. N. DATTA, editor. *New Light on the Punjab Disturbances in 1919: Volumes 6 and 7 of Disorders Inquiry Committee Evidence*. Simla: Indian Institute of Advanced Study. 1975. Rs. 125.00; 75.00.

Punjab Disturbances, 1919-20. Volume 1, Indian Perspective; volume 2, British Perspective. Reprint. New Delhi: Deep Publications. 1976. Pp. 159; lvi. 275. \$12.00 each.

Many events influenced the final resolution of the conflict between the British rulers of India and their nationalist opponents, but none was more dramatic than the widespread Punjab disturbances in the spring of 1919. On April 19th the British army surrounded a large assembly of protesters at a large public garden, Jallianwala Bagh, and killed or wounded thousands of unarmed Punjabis. The army and civilian administrators followed the massacre with brutal repression of agitators and suspended civil liberties in several districts. The sometimes peaceful and sometimes violent confrontation stunned British and Indian politicians. The Government of India launched an investigation, the "disturbances enquiry committee" headed by Justice Hunter, and the Indian

National Congress countered with its own inquiry. Not surprisingly, the Hunter Committee emphasized the British justification for reliance on force, and despite limited criticism of tactics, generally supported official policies. The Congress study in turn challenged British assumptions about the necessity for repression and pinpointed numerous instances of "zulam" (atrocities or brutality by officials).

The details and the larger issues surrounding these events remain controversial to this day. The subsequently published accounts by British civil servants—such as the head of the Punjab government, Michael O'Dwyer—maintained that severity alone prevented a massive uprising, a position also reflected in the Hunter report and in the five volumes of official evidence published in 1920. Indian participants told a different story. In recent years, the massacre or the broader developments in 1919 have generated a range of biographies, propaganda pieces, and scholarly studies.

The value of the publications under review lies not in the area of startling new evidence (although the "new light" volumes do present for the first time data unavailable outside of hitherto confidential files) but rather in providing scholars and general readers with a complementary set of reports usually found only in the most specialized research libraries. V. N. Datta, author of a balanced monograph on the disturbances, has discovered and now published volumes of evidence suppressed by the Hunter Committee. Among the most valuable of the documents is an extensive statement (500 pages) by the Punjab government on the political decision-making and events surrounding "the uprising." Datta has enhanced the value of the two volumes by supplying a contextual statement and a short evaluation of contents. The two reprints present both perspectives in accessible form. The Hunter report examines virtually all major issues from a majority point of view, followed by a separate report by three Indian members less sympathetic with British intentions. Appended are a useful chronological table, miscellaneous documents, and maps.

Although investigating all Congress-related disturbances, including those in Delhi and Bombay, the Hunter material focuses primarily on Punjab. The Congress committee, which met with 1,700 witnesses and published 650 statements in a separate evidence volume (not reprinted), covered much the same ground but in light of Indian reactions and aspirations. Every library with an interest in India or imperial politics should have these two sets of published documents.

JERRY BARRIER
University of Missouri,
Columbia

G. W. CHOUDHURY. *The Last Days of United Pakistan*. Bloomington: Indiana University Press. 1975. Pp. xiv, 239. \$10.00.

SAFDAR MAHMOOD. *A Political Study of Pakistan*. Lahore: Sh. Muhammad Ashraf. 1972. Pp. vi, 395. Rs. 20.

The central theme of both these books is the disintegration of Pakistan in December 1971. The authors try to explain the same events from different perspectives and therefore assign different weights to the central causes that led to the breakdown of the state. Political scientists usually formulate their theories of political systems in terms of their maintenance. These two political scientists make heroic attempts to explain the breakdown of a political system, but neither of them claims to be a theorist, and no theory of a system breakdown emerges from their writings.

G. W. Choudhury was a minister in the Pakistan government during 1969–71 when some of the crucial negotiations between President Yahya, Sheikh Mujibur Rahman, and Z. A. Bhutto took place. Prior to that he had served as Director-General of Research in the Ministry of Foreign Affairs; he therefore claims that this is an insider's view of the tragic events that led to the disintegration of Pakistan. Choudhury also drafted the Legal Framework Order (LFO). He asserts that since the LFO stated that disparity between the two parts of Pakistan should be eliminated by statutory provisions to be guaranteed in the constitution, and that the provinces would enjoy the "maximum degree of autonomy consistent with giving the central Government adequate power to discharge its federal responsibilities," Mujibur Rahman should have accepted the LFO and modified his Six Points (which were secessionist). So far the argument is pretty straightforward. But while he has advantages over an outsider because of his inside information, he lacks the objectivity of an outsider because of certain built-in biases compounded by the fact that Mujibur Rahman and Bhutto were responsible for wrecking his plan as stated in the LFO. The result is that Choudhury's logic becomes distorted; he cannot understand why Mujib did not trust Yahya, when the causes of this distrust are apparent. Choudhury also states in his book that Yahya was "never master in his own house"; yet it was clear to Mujib that Yahya could not control his own generals and that the generals distrusted Mujib intensely for his involvement in the Agartala Conspiracy Case. Jinnah, the founder of Pakistan, used to ridicule his Congress opponents by saying "the spider (the Congress) tells the fly to walk into my parlor and when the fly refuses, the fly is called unreasonable or intransigent."

sigent." Similarly, as a political scientist Choudhury knew that Mujib was presiding over a coalition in the Awami League which was dominated by separatist hawks. Therefore, he could not reach a settlement with the Yayha regime merely on the basis of imprecise assurances of goodwill and generosity. Given the defense problems of Pakistan and the dominance of the military and bureaucratic elites in the policy-making process, Mujib and his followers knew that neither the removal of economic disparity between the two wings nor the demand for maximum autonomy for East Pakistan would ever become a reality. Choudhury himself argues that Bhutto was so consumed by his ambition to become the dominant leader in West Pakistan that he probably welcomed the possibility of East Pakistan's separation. Given all these factors, how could Mujib walk into the spider's parlor?

The problems and events leading to the separation of East Pakistan also constitute the most important theme in Safdar Mahmood's *A Political Study of Pakistan*. Mahmood subscribes to the conspiracy theory, namely that the role played by Hindus, Communists, India's subversive policies, the CIA, and Russian agents all contributed their respective share toward the eventual dismemberment of the country. The final blows were struck by Indian forces supported by Russian military and political assistance. He is prepared to concede that economic disparity and the underrepresentation of the Bengali majority in the power structure caused considerable havoc, but all these flames were really fanned by the Indians. "Figures and data regarding export earnings of jute, rate of G.N.P., per capita income, rate of industrialisation in East Pakistan and its share in Services were processed and framed in research centres established by the Indian Government in Calcutta and were then distributed as pamphlets amongst different agencies in East Pakistan."

Both Choudhury and Mahmood agree that the Russians played a significant role in the breakup of Pakistan. Russian policy was motivated by that country's annoyance with Pakistan not only for having allied itself with China but also for having played a role in bridging the gulf between China and the United States. But since Mahmood has become the Deputy Director of Intelligence only recently, he does not claim access to inside information. He does, however, assert that Russian animosity toward Pakistan continues in the sense that the Soviet Union "is maintaining close contacts with some of the tribes in Baluchistan and N.W.F.P. and rumour persists that guerillas are being trained in Russia."

KHALID BIN SAYEED
Queen's University

LEA E. WILLIAMS. *Southeast Asia: A History*. New York: Oxford University Press. 1976. Pp. xiii, 299. Cloth \$12.95, paper \$5.50.

Lea Williams has "tried to take a fresh approach" to Southeast Asian history "by paying more attention to historical themes and currents than to individuals and events." His goal "is to offer readers an analytical framework . . . to generate an awareness of the unity in diversity that is Southeast Asian history." What Williams has done, in fact, is write a literate, and, in some ways, old-fashioned history. Despite his concern that "specialists are likely to be outraged by what may seem to be the cavalier treatment afforded their chosen periods, places, and people," most current specialists are more likely to be puzzled by this return to a neo-traditionalist vision of a Southeast Asian dialectic. This text is Euro-centric and represents a swing away from the Asia-centric focus so prevalent since Jacob Cornelius van Leur's seminal essay "On the Study of Indonesian History."

This point is clearly demonstrated by the allocation of space and even by the chapter titles: The Setting, Cultural Crossroads, The First Wave of Imperialism, The Second Wave of Imperialism, The Impact of Modern Colonialism on Maritime Southeast Asia, The First Centuries of Imperialism in Continental Southeast Asia, The Final Imperial Conquests, Colonialism in its Prime, Nationalist Evolution, Japanese Interregnum, Nationalism Triumphant, Independent Paths, and Southeast Asia in Global Politics. After a brief, cohesive introduction, Williams' book really starts in 1511. The Portuguese conquest is given far more attention than the spread of Theravada Buddhism. Rajah Brooke's swashbuckling story is retold, but "placid and poor, the Malay States slumbered on."

The dialectic of European thesis and Asian antithesis often obscures some of the nuance of Southeast Asian history. Thus Williams' treatment of the opening of the Burma delta minimizes the role of the Burman freeholder in the process of economic development while emphasizing that of the alien Indian. Far more attention is paid to the external struggle by the Indonesians against the Dutch than to the autonomous, internal development of an Indonesian sense of national self. Unlike earlier, Euro-centric historians of Southeast Asia, however, Williams is sharply critical of imperialism and of the Europeans. The spread of Christianity in the Philippines is described as attaining "the proportions of an epidemic." The Dutch construction of Batavia, the author notes, remains a monument "to man's ability to befoul his environment." And in his scathing and accurate recounting of the Vietnam war, Williams summarizes American intervention in Cambodia: "the Ameri-

cans had snatched defeat out of the jaws of stalemate."

This book is an extended essay on Southeast Asian history. Virtually free of footnotes and with many photos taken by Williams himself, it returns to a traditional framework for understanding Southeast Asia's past. It is the distillate of many years of teaching at Brown University and is both lucid and personal. It is a work that should be read.

DAVID J. STEINBERG
Brandeis University

RICHARD ELY. *Unto God and Caesar: Religious Issues in the Emerging Commonwealth, 1891-1906*. (Studies in Australian Federation.) Melbourne: Melbourne University Press; distributed by International Scholarly Book Services, Forest Grove, Ore. 1976. Pp. x, 162. \$20.00.

The doyen of Australian historians Manning Clark has often complained that writers of Australian history tend not to ask the big questions or deal with those less tangible aspects of life that so often affect human behavior. Religious belief is one major area of human existence where the rational and the irrational constantly jockey for paramountcy. Unfortunately, religious history including the study of the impact of belief on political behavior has tended in Australia to remain the province of specialist religious historians and many useful insights have failed to flow on to historians dealing with other facets of the country's development.

In this book Richard Ely goes some way toward rectifying this imbalance. It is, to speak plainly, a very good book. First, because it fills a major gap in our knowledge of the conflicting forces which, in 1901, procured a federated Australia; and second, because Ely makes it very clear that the success of the referenda that finally established an Australian nation may have hinged quite crucially on religious issues rather than on the more prosaic economic and political motivations.

The early chapters deal with that old bogey of Australian history, sectarianism. Ely recounts the failure of Roman Catholic leader Cardinal Moran to override the sectarian boundaries separating the various creeds in Australia and to have himself accepted as a "Christian" rather than a "Catholic" spokesman. Protestant reaction was both swift and compelling, and in the elections held to select ten candidates from New South Wales to attend the Federal Convention, Moran came in fourteenth. The figures indicate that Moran was fundamentally out of touch with political reality and furthermore that many Roman Catholics had also voted against him. One of the unintended

consequences of Moran's candidature was that it considerably heightened both lay and clerical awareness of the developing momentum toward federation. After such an open rebuff Moran decided to lay low for a time and left the public arena.

Ely then moves on to discuss the role of the Seventh Day Adventists and to delineate the outlines of what became a major conflict embracing Protestants, atheists, and agnostics throughout Australia over the formal recognition of God in the new constitution, and the specific powers over religion that would be vested in the new federal government. In this conflict the Adventists made good use of their American experience and American personnel, and provided yet another area of the federation proceedings in which America loomed large.

Finally, and perhaps most usefully of all, Ely produces a trenchant indictment of the immensely respected and previously authoritative *Annotated Constitution of the Australian Commonwealth*. All in all, this is a refreshing and stimulating book. It would be a pity if its title were to dissuade all but specialist religious historians from reading it.

F. G. CLARKE
Macquarie University

C. Y. CHOI. *Chinese Migration and Settlement in Australia*. Sydney: Sydney University Press; distributed by International Scholarly Book Services, Portland, Ore. 1975. Pp. xiii, 129. \$12.25.

This is a demographic survey of the subject, with emphasis on the period since 1947. The historical demography of the Australian Chinese is broadly similar to that of their compatriots in Canada and the United States; Australian immigration policy and political conditions in China have been the major forces determining the size and character of Chinese immigration and settlement.

C. Y. Choi combines an historical survey with summary results of his own social survey research. The result is a brief, rather modest, but very useful introduction to the subject. The methodological heart of the book is in chapter six, where the author introduces demographic material from a Chinese temple in Melbourne, allowing him to pinpoint village-of-origin for a significant sample of Melbourne's Chinese immigrants from 1893 to 1913. The same material makes possible an analysis of surname distribution by village in China which, in turn, allows the author to illustrate the pattern of locally-intensive, personally-linked immigration ("chain migration") that Australian policy and Chinese culture combined to bring about. Would that similar data were available for

all countries where Chinese immigration has occurred!

EDGAR WICKBERG
University of British Columbia

UNITED STATES

ROBERT BELLAH. *Beyond Belief: Essays on Religion in a Post-Traditional World*. New York: Harper and Row. 1970. Pp. xxi, 298. \$4.95.

ROBERT BELLAH. *The Broken Covenant: American Civil Religion in Time of Trial*. New York: Harper and Row. 1975. Pp. xvi, 172. \$7.95.

In 1967 Robert Bellah published "Civil Religion in America," a perceptive essay that posited the existence of "an elaborate and well institutionalized civil religion," and noted the many ways in which Americans have participated in its rites. Most remarkable, however, was his claim that "few have realized" that this religion existed. In fact, the scholarly literature was vast, with the most recent surge having arisen in the 1950s when the Congress, with strong support from President Eisenhower, added "under God" to the pledge of allegiance and made *In God We Trust* the national motto. Will Herberg's *Protestant-Catholic-Jew* was perhaps the most noticed work of that period. Slightly preceding Bellah's article was Sidney E. Mead's "The Nation with the Soul of a Church," which took its title from a much earlier work of G. K. Chesterton who, like Bellah, traced the American civil "creed" to the Declaration of Independence.

Yet despite this widespread interest, Bellah did spur the discussion to a new order of magnitude, almost like a skier making a new trail who releases an alpine avalanche. Given this large response, Bellah returned to the fray far more often than can be noted here. In "American Civil Religion in the 1970's," for example, he analyzed Nixon's Second Inaugural and McGovern's campaign rhetoric. (*Anglican Theological Review*, July 1973) Perhaps most important of these ripostes was "Civil Religion and the American Revolution" (J. E. Brauer, ed., *Religion and the American Revolution*, Philadelphia: Fortress Press, 1976). Here in a bicentennial context serious uncertainties emerge. He opens with the assertion that "there is a sense in which the American Revolution and the civil religion are one." But toward the end he observes that "the tenets of the civil religion" are not self-evident. Recognizing the biblical and utilitarian traditions as "the two great structures which have been behind the civil religion from the beginning," he concedes that "conflict, explicit or implicit, . . . has been endemic. The conflict over the deeper

meaning of the civil religion, over the very meaning of America, has never been more severe than it is today." There are also rumors that he now regards Rousseau's "civil religion" terminology as confusing and unnecessarily controversial. In any case the crucial concept awaits its Aristotle. Bellah, nevertheless, has renewed our concern for a vital aspect of American public life with a persistent and valuable emphasis on the religious dimensions of the nation's social ethics.

A fuller account of his methods and principles is given in two recent works. *Beyond Belief* contains the original civil religion essay, an autobiographical account, and essays on sociological method and the study of religion. *The Broken Covenant* is a critical interpretation of the American experience. Like H. R. Niebuhr's *Kingdom of God in America*, it is cast in an historical form. It moves from early colonial times to the present by way of six chapters: Myths of Origin, America as a Chosen People, Salvation and Success, Cultural Pluralism, The Taboo on Socialism, and The Birth of New American Myths. His hopes for America rest chiefly on the Puritan notion of Covenant, the *Caritas* ideal of Governor John Winthrop, and the idea of republican virtue which this ideal fostered. The most corrosive elements in American life are rationalistic utilitarianism and the lust for wealth. Bellah's goal is a free society that recognizes justice and the general welfare as national necessities. His most serious historical shortcoming (odd for a sometime Marxist and admirer of both Max Weber and F. O. Matthiessen) is an overly intellectualist mode of historical explanation and a failure to perceive the process by which American colonial society gradually became the world's prime example of the bourgeois revolution, and thus the degree to which a competitive and exploitative outlook had already taken root in the seventeenth century. The very least that can be said is that Bellah's work deserves the gratitude and continued concern of historians.

SYDNEY E. AHLSTROM
Yale University

CHARLES HUDSON. *The Southeastern Indians*. Knoxville: University of Tennessee Press. 1976. Pp. xviii, 573. \$23.50.

If inscribed symbols are to be accepted as the exclusive sources of history, *The Southeastern Indians* would have to be regarded as merely prolegomena to a history that hardly existed before Europeans intruded and recorded. By a different definition, however, this is history on the grand scale, tracing the origins, growth, and decline of a large social system. For sources, Charles Hudson has had to depend heavily on archeology and ethnology. He is

an anthropologist by training, but he has adopted the purposes of the social historian and he understands the working of time upon cultures.

Although Hudson gives a complete description of the culture shared by the aboriginal tribes of what is now the Southeastern United States—sometimes, indeed, telling more than I cared to know—his book is ethnohistory rather than simple ethnology. It ranges from the most ancient Stone Age in America through the evolutions of the Woodland and Mississippian cultures to the edge of the times of written records, and it describes in precise detail what happened to aboriginal societies as a result of contact with Europeans. It fills a hitherto vacant niche. The Indians of the Southeast have been rather neglected by historians and anthropologists, and this book was overdue. Now, happily, it has been done so splendidly that future historians of the region, and of Indian societies generally, will have no option but to master it.

For historians habituated to standard forms of presentation, mastery may require conscious re-orientation, as *The Southeastern Indians* is organized unconventionally in a form like a well-filled sandwich. Two chapters introduce the region and trace its social evolution from the earliest indications of human habitation to the traumatic moment of European intrusion. Five more chapters analyze Indian society as of that time—what might be called the aboriginal climacteric—after which a final chapter hurries through a synopsis to the present day. That final chapter is included for perspective; Hudson promises its expansion into future books.

The meat in this sandwich is the material on the society's fullest development in the sixteenth-century, before European diseases, guns, trade goods, and persons transformed the habitat. Hudson regards the culture of that era as a number of local variants formed upon the mold of a belief system generally common to the region. The categories of definition prescribed by the belief system, although they were "the fundamental stuff of all belief systems, whether preliterate, like that of the aboriginal Southeast, or literate, like our own," were distinctive to the region and to the preliterate way of thinking. In the latter sense, these categories extended from the realm of the supernatural to include the phenomena of the natural world. It is oddly almost as though Feuerbach and Marx had been stood upon their heads: the Indian "heaven" creates earth in its own image. It is not quite Genesis, however; the Indians were more modest than to make themselves mirror the Great Spirit. They found more immediate kinship with the animal world.

Their belief system controlled the Indians' conceptions of the material world and prescribed the rituals of their mode of adaptation. Hudson de-

scribes subsistence practices and systems of social organization as well as those of ceremonies, arts, and recreation. He has written good English prose, avoiding jargon and explaining necessary technical terms, and his illustrations have been carefully chosen to demonstrate, rather than merely ornament, the text. It is a readable and handsome book.

FRANCIS JENNINGS
The Newberry Library

R. C. SIMMONS. *The American Colonies: From Settlement to Independence*. New York: David McKay Company. 1976. Pp. viii, 438. \$14.95.

This volume, a survey of the settlement, growth, and eventual independence of the British colonies in America, may eventually be considered significant mainly as an expression of the recent conflicts of interpretation among New Left and Consensus historians. R. C. Simmons states that he has attempted "to combine a narrative introduction to early American history with the findings of recent scholarship," but it is quite evident that his sympathies lie with New Left rather than Consensus views, although both are relatively recent. This would be perfectly acceptable except that his facts do not always lead to New Left conclusions.

Let us take the structure of colonial society as an example. The terms "deference" or "deferential," "elite," "propertied slaveholding elite," "local ruling class" of wealthy men, "gentry class," "settled social elite," "Quaker oligarchy," and "New York's ruling class" all indicate a New Left (and Old Left) view of a class-structured society. But then Simmons shows that North Carolina had a "popular assembly-based group" in conflict with the governor, that Penn referred to Pennsylvania as "this licentious colony" and his opponents dominated the assembly by raising the "popular and plausible cry of standing for the liberties and privileges of the people," that expansion of wealth benefited many, that land was cheap and labor was expensive, that peasants in German churches desired to play major roles and "everything depends on the vote of the majority," and that members of the lower houses were elected by potentially wide electorates. He even cites Franklin and Chastellux on the middle-class democratic nature of colonial society.

Given Simmons' depiction of colonial society as elitist, one would expect to discover that the American Revolution was a social movement, but such is not true. Simmons rightfully says that historians have placed too much stress on conflict and not enough on consensus. As he shows, the Revolution brought very little social change—almost as

though colonial society were a middle-class democracy objecting to the threats posed by British imperialism.

There are many similar instances, but Simmons' treatment of patriots and loyalists provides a particularly good example of the need for historians to get their facts straight before they interpret them. The author is quite correct in saying that patriots and loyalists represented the same social classes, but then he errs in his estimate of numbers. He cites the famous John Adams statement that one-third were patriots, one-third were loyalists, and one-third were indifferent, a statement that referred to American attitudes toward the French Revolution, not the American war. And if, as he says, there were 60,000 to 100,000 loyalists out of a population of 2,000,000, this would be only three to five percent, which corresponds to John Adams' estimate that nineteen out of twenty Americans adhered to the American cause.

When Simmons avoids the injection of ideology, he produces some good history, but classroom teachers will find the organization of material difficult for students, and historians involved in the New Left-Consensus controversy will doubtless evaluate the book on the basis of ideology.

ROBERT E. BROWN.
Michigan State University

B. R. BURG. *Richard Mather of Dorchester*. Lexington: University Press of Kentucky. 1976. Pp. xiii, 207. \$15.95.

This book offers fresh evidence, if any is needed, that the study of Puritanism remains a lively enterprise. As a work of history, it adds to our knowledge of ecclesiastical polity and of church-state relations in New England, no mean achievement considering how intensively those subjects have been studied. It also contains a suggestive discussion of the preaching of a minister to a village, and the village itself receives treatment as a "peasant society" in an interesting analysis that draws on recent work by anthropologists.

The book is less effective as biography. B. R. Burg argues that Richard Mather never influenced the destiny of nations or determined the fate of even a small portion of mankind, "living in 'obscurity,' an ordinary clergyman, similar to many in New England." It is true that Mather resembled many ministers in Massachusetts, but as one reads this book his differences from most, especially the public roles he played, seem more impressive. He did not have John Cotton's or Hooker's intellect, but he took part, as most ministers did not, in the resolution of the crisis of the first generation over church polity and baptism.

Whether he moved Western civilization is beside the point; the fact is he published defenses of the "New England Way" (most did not), he helped write the Cambridge Platform (most did not), he contributed to the definition of the Halfway Covenant (most did not), and he attained a degree of public recognition, for example, appointment as a Harvard overseer (most did not).

If Burg did not construct much of his interpretation of Mather's behavior on this assumption of "ordinariness," there would be no point in even mentioning these matters. But he does frame his interpretation on this assumption, describing Mather as a climber, a status-seeker who aspired apparently to escape a low estimate of himself and to gain acceptance by elites in England and America. One reverse in his ascent, the initial failure in Dorchester to form a church under his guidance, seems of critical importance to Burg, who describes it as engendering a rage that lasted through the remainder of Mather's life. The "humiliation" Mather felt, according to Burg, strongly contributed to his beliefs and attitudes, especially his revulsion against the state's interference in the forming of churches. The evidence of Mather's inner reactions is, of course, scant indeed, and Burg has resorted to rather extravagant speculation. He has probably also underplayed the lead taken by the ministers, most notably Thomas Shepard, in the Dorchester episode. (On page 112, a crucial passage has the magistrates refusing Mather permission to gather his church—with no mention of the ministers.)

If Burg has strained the evidence in this explanation of Mather's motives, he has used it skillfully in his analysis of the Cambridge Platform and the Halfway Covenant. In fact the two chapters given to these topics—almost half the book—are excellent. They include a fine reconstruction of the work of the Synod of 1646-48 and a shrewd assessment of Mather's contribution. The account of the Halfway Covenant possesses a comparable freshness. Burg uses anthropological theory in analyzing the data he has accumulated on what is known of the citizens of Dorchester and offers interesting suggestions about the relationship of landholding to attitudes toward the Halfway Covenant. This is done with caution, tentativeness, and respect for the limitations of the sources.

All in all, though Burg may offer questionable interpretations of personality, he provides the fullest account we have, and are likely to have, of the life of Richard Mather. And in the process he has also written a fine history of the early years of the Bay Colony.

ROBERT MIDDLEKAUFF
*University of California,
Berkeley*

DONALD JACKSON and DOROTHY TWOHIG, editors. *The Diaries of George Washington*. Volume 1, 1748-65; volume 2, 1766-70. Charlottesville: University Press of Virginia. 1976. Pp. lv, 373; xvi, 374. \$15.00 each.

Donald Jackson and Dorothy Twohig have accomplished a unique feat in the republication of Washington's diaries. With art and scholarship they have complemented the spare entries of a practical diarist with annotations, illustrations, portraits, and maps so that the sum is greater than any of the parts. The volumes represent a monument to creative editing at a time when some are apprehensive about the finances and the editorial scope of letterpress publications of the papers of American public figures. The new Washington project is vulnerable to criticism from those seeking greater economies in publication or heavier emphasis upon lesser known figures. Because of the number of previous editions of Washington's papers and diaries together with the numerous secondary works, some will question the merits of yet another edition of his papers. Despite this voluminous preoccupation, most agree that Washington remains an enigmatic figure. While these volumes might not quiet the disputes concerning the future course of historical editing, they accomplish their stated purpose to make Washington, his acquaintances, and his friends "come alive." Such an achievement should influence the current editorial dialogue, but, more importantly, it will stand as a distinguished contribution to our understanding of Washington and to the popularization of American history.

Unlike some of his contemporaries, Washington did not keep a diary for the edification of future generations or as an intellectual sounding board but rather as a repository for practical and social notes and reminders. There were notable exceptions, particularly the journal he kept as an agent for the governor on his first western trip and his account of the expedition against the French in 1754. The editorial treatment of these travels is superb. The maps are located perfectly in the text and composed with graphic clarity to aid the reader in following Washington's tracks. To complement Washington's 1753 journal, the editors have added pertinent excerpts from the diary of Christopher Gist who acted as guide on the trip and whose comments add both spice and weight to Washington's observations.

Before he was a general or a president, George Washington was a planter, and his comments illuminate his management of a diversified agricultural conglomerate. Between 1761 when he inherited Mount Vernon and 1770, Washington tripled the size of his home tract through trades and pur-

chases. The editors synthesize these acquisitions with a map and chart which give Washington's interest in land investment a cohesive sense of space and time. Economic historians will be disappointed that the acreage chart omits prices, but conditions of sale are included in footnotes for those diary entries commenting upon purchases. To meliorate understanding of Washington's agriculture, the editors have provided horticultural descriptions of plants and technological data about tools and the construction of grain mills. When these editorial observations are added to Washington's commentaries about crop rotation and yield, land use, land investment in the West, animal breeding, labor use, and product marketing, a clearer picture of the problems and prospects of large-scale farming in the eighteenth century emerges.

This edition will interest social and political historians as well. The editors have identified as many of the people mentioned in the diary as possible and indicated their connection with Washington. As Washington began to travel more frequently to Williamsburg to transact family business and to attend the legislature, the editors insert useful material about contemporary political developments and social events. His political behavior during these years has received sufficient historical treatment so that readers of the diary will not interpret the absence of political commentary as a lack of his concern. On the contrary, his diaries indicate that amid the mounting imperial crisis Washington retained an active interest in his many public duties and in his private farming interests. The editors intrude only in such a manner as to augment an appreciation of the complexity of Washington's maturing character and his varied activities during a time of growing public unrest. At no point, however, do the editors eclipse Washington's presence. Jackson and Twohig have graced Washington's words with an imaginative editorial deftness which grants the reader a rare audience with America's first citizen.

GEORGE M. CURTIS III

The Papers of John Marshall

ARTHUR BERNON TOURTELLOT. *Benjamin Franklin: The Shaping of Genius, The Boston Years*. Garden City, N.Y.: Doubleday and Company. 1977. Pp. xviii, 459. \$10.95.

It is customary to think of Benjamin Franklin primarily as a citizen of Philadelphia, where he spent so much of his life establishing his career and doing the things that formed his reputation. Yet in Arthur Bernon Tourtellot's *Benjamin Franklin: The Shaping of Genius, The Boston Years* we are re-awakened to the essential fact that Franklin was always

the product of his Boston origins. Whatever other influences came later, it was the Boston and New England experience in the early years of the eighteenth century that molded him and stimulated his mind. Absorbing much from the spirited life of the small seaport and the turbulent forces working in a society that was still Puritan but was becoming secularized and prosperous, Franklin found in the conflict between the old and new exactly those influences needed to encourage his natural talent and intelligence. And out of the tension and conflict he got the measure of his philosophy, learning the value of study and inquiry and a distaste for Puritan dogma and authority. When he left in 1723, he carried away "most of the pragmatism and none of the dogmatic content of Puritanism."

Franklin was early and thoroughly exposed to Boston's intellectual ferment. His father, Josiah, was both a tallow chandler and a respected and consulted member of Old South Church on easy and familiar terms with the leaders of the community. In this father's house he met men like Samuel Sewall and the Mathers and was "wholly a product of Cotton Mather's Boston, with its contradictory reliance on curiosity and intellect and its stubborn insistence upon the orthodoxy of the faith."

We learn a great deal about Franklin's education, how his own curiosity led him to books and reading and then to questioning the stern precepts of his elders. It was from books that he found the sources to discover the outer world where reason and enlightenment were emerging guides for human behavior. Tourtellot tells us about Franklin's short formal education, his boyhood associates, and personal interests like his love of swimming. He covers Benjamin's entry into the printing trade and his unhappy relationship with his brother James. But beyond these obvious topics, we are led to valuable digressions that inform us about how tools worked or everyday tasks were done, about the peculiar practices and customs of the time, and, especially, about the people Franklin encountered in his daily activities. This is an immensely rich book in the texture and variety of a town and society that was very English and distinctively Puritan. One can almost smell the sea and hear the waves splashing against the Boston piers. More than a study of a life, this biography displays an intimate knowledge and feeling for another time and place, enabling the reader to see the working relationship of an individual and his society. Our appreciation of Franklin's genius and its sources is now all the greater because of the author's ten years work on this book.

WILLIAM S. HANNA
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DON HIGGINBOTHAM, editor. *The Papers of James Iredell*. Volume 1, 1767-1777; volume 2, 1778-1783. Raleigh, N.C.: Division of Archives and History. 1976. Pp. xc, 500; xxv, 508. \$16.00.

For over a hundred years the only available edition of the papers of the North Carolina patriot and United States Supreme Court Justice James Iredell has been Griffith J. McRee's *Life and Correspondence of James Iredell* (1857-58), a two-volume work marred not only by the expected errors of nineteenth-century scholarship and Victorian family editing, but also by a blatant sectionalism that led McRee to structure his volumes as a defense of the antebellum South. Thus, historians and legal scholars have ample reason to welcome Don Higginbotham's splendidly edited two volumes, the first of four that will constitute a complete new edition of Iredell's papers. "A somewhat separate set" within themselves, volumes 1 and 2 contain correspondence, political essays, and other documents dating from Iredell's 1767 arrival in Edenton, N.C., as a seventeen-year-old royal customs official to the end of the War of Independence, in which, despite his youth and position, he was a leading North Carolina revolutionary. Still to appear are volume 3, also edited by Higginbotham, presumably covering Iredell's later life, including his nine years on the Supreme Court, and volume 4, edited by John E. Semonche, "devoted exclusively to legal papers."

In these first two volumes Higginbotham deals well with the textual problems posed by the varying nature of the materials, and users will be pleased by the degree of completeness found here. "Only a few routine business papers and memoranda" have been calendared or omitted, and in the case of the properly calendared records of the Port of Roanoke, he provides a brief, but informative analytic summary.

Nevertheless, to view Higginbotham's work merely as a correcting and augmenting of McRee's edition is to miss its full value. The thoughtful introductory essay, "The Making of a Revolutionary"; the comprehensive scholarly notes; and the inclusion of several well-chosen letters, not to or from Iredell, but relating to his activities, make these volumes more than another valuable reference tool for scholars. They are also a window on the fascinating social and political world of eighteenth-century North Carolina and with volume 3 will make an effective substitute for the major modern biography that Iredell lacks.

Indeed, as presented here the Iredell papers reveal clearly his profound impact on Revolutionary North Carolina and the unavoidable irony of his role, the "irony . . . of a king's official servant spearheading one of the crown's colonies down the

road to rebellion and independence." Meticulously maintaining his custom records as late as April 1776, he nevertheless wrote a series of eloquent essays which, expounding a somewhat original analysis of constitutional issues, provided Carolina patriots intellectual justification for moving toward revolution. Iredell himself was, as Higginbotham terms him, "a reluctant revolutionary," because a break with the mother country meant a break with his family there and loss of a substantial inheritance. Not until the spring of 1776 did the logic of his own political arguments and his intimate Carolina connections impel him to join "in the noblest of all causes, a struggle for Freedom."

As excellent as Higginbotham's insights into Iredell and his times are, this reviewer wishes that he had directed the attention of users more to Iredell's religion, a factor as important in his development as was his political thought. Although Iredell accepted natural rights ideas, he was no deist, nor was he an evangelical enthusiast. Rather, he was a simple believer with an extraordinarily strong sense of Christian duty. It was Iredell's faith, one suspects, that enabled him to make his political transition with unusual grace and honesty, avoiding the horns of his unique personal dilemma.

PHILANDER D. CHASE
The Papers of George Washington

ROBERT L. FERM. *A Colonial Pastor: Jonathan Edwards the Younger, 1745-1801*. Grand Rapids, Mich.: William B. Eerdmans Publishing Company. 1976. Pp. 214. \$7.95.

Jonathan Edwards, Jr., was an almost classic epigone. Before he was thirteen years old he had seen his father lose a prominent pulpit, serve a frontier mission, accept a college presidency, and die an early death. In pursuit of his own ministerial vocation, he earned a Princeton degree and read theology under his father's closest associates, then settled into a parish in New Haven. Twenty-six years later he was dismissed, "exiled" to a remote congregation, and in 1799 installed as president of Union College. He died soon afterward—though not before he had brought about a rapprochement between Congregationalists and Presbyterians in the famous Plan of Union. Notwithstanding the propitious times through which he lived, he is remembered chiefly as an apologist for the "New Divinity," an abstruse system of doctrine struggling to maintain "consistent Calvinism" against eroding belief in human depravity, predestination, and eternal punishment. Lacking the metaphysical subtlety that distinguished his father's thought, "Edwards' theology was rigid and legalistic and basically unacceptable to an age which was confident of its power, freedom, and self-deter-

mination" (p. 175). Little wonder that the younger Edwards has attracted few historians: he plainly was an anachronism.

So, unfortunately, is this book. As theological history in the old style, it expounds ideas in a narrow ideological matrix and pays insufficient attention to cultural context. Had it been published in 1958, when it was presented as a Yale dissertation, it doubtless would have been better received. Now, however, most students of such subjects strive for more sophisticated treatment in terms of sociology of knowledge and psychological analysis. (A sixteen-page bibliography lists only nine works published after 1958, and none of these appears to inform the book in any significant way.) Robert L. Ferm claims that Edwards' biography "illumines our understanding of his age" (p. 171), but the light scarcely shines beyond Edwards' own theological circle. What this book mainly conveys is the dreary pedantry of the New Divinity; and if one has to know about such matters, Ferm's brief summary is competent. But Edwards himself, as a jacket blurb warns, remains "still somewhat elusive."

C. C. GOEN
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HOWARD MILLER. *The Revolutionary College: American Presbyterian Higher Education, 1707-1837*. New York: New York University Press. 1976. Pp. xxiii, 381. \$15.00.

This boldly conceived and well-written study illuminates a century of Presbyterian higher education by casting it against the background of evolving Presbyterian ideology and ecclesiology. The Presbyterian colleges of the eighteenth and early nineteenth centuries, Howard Miller contends, were "institutional manifestations of crucial changes within American Presbyterianism." Utilizing this conceptual framework, Miller persuasively develops his central thesis. American Presbyterianism, he argues, underwent a fundamental transformation between 1730 and 1820 in which its leaders abandoned their vision of an organic, Christian commonwealth "free of selfishness and contention" and accepted, if haltingly, the view that society was composed of several denominational communities which competed legitimately with one another "with no obvious concern for the 'common good' of society." Presbyterian higher education mirrored this transformation. Whereas mid-eighteenth-century Princeton had been an expansive, integrative institution that reached out to embrace a broad spectrum of Christians, by 1820 Princeton and such Presbyterian colleges as Dickinson, Transylvania, Centre, and Hampden-Sydney had become "bastions of Presbyterian orthodoxy and important weapons in the

sectarian competition." These were the truly "revolutionary" colleges, reflecting as they did the eclipse of an organic view that "had sustained American society from its inception."

The Revolutionary College has many strengths. Miller uses his extensive research in church and college records to argue a thesis that is carefully defined yet refreshingly comprehensive. The result is to provide new insights into both early American religion and higher education, insights of the kind that one rarely finds in studies of individual colleges. The book is also crisply organized. In each of three broadly chronological sections, Miller first analyzes a major stage in the evolution of American Presbyterianism, then examines how Presbyterian colleges reflected that stage: in purpose, control, financing, pedagogy, curriculum, and discipline.

Yet, one notes questionable uses of evidence on occasion. Pity poor William Smith, the Anglican minister and educator. Twice Miller discusses Smith's 1752 pamphlet, *Some Thoughts on Education*, as if it were written by William Smith, Jr., the Presbyterian lawyer, a difference of some consequence. Miller also turns the Anglican Smith's proposals for the "College of Mirania" into a rigidly aristocratic plan of education (which he compares with integrative Presbyterian ideas) by insisting that Smith wanted to divide students according to social class, whereas Smith actually proposed dividing them according to prospective occupation. Nor is Miller on solid ground when he emphasizes the interdenominational makeup of early Princeton's trustees. Virtually all of them were Presbyterians, including twenty of the twenty-two appointed in the charter of 1748 and the entire board by 1756. The contrast with the trustees at the denominational colleges after 1800 is rather less decisive than Miller would have it. As a final example, it is at the least debatable whether the educational writings of William Livingston, a man of extremely latitudinarian religious beliefs, can be accurately portrayed as an expression of New Side Presbyterian views on higher education.

In the last three cases and elsewhere in the book one detects a tendency to interpret the evidence to suit the thesis. Yet, *The Revolutionary College* remains an essentially sound work, distinguished by an imaginative approach, enlightening conclusions, and many an intriguing point along the way.

DAVID C. HUMPHREY

Lyndon Baines Johnson Library

C. A. WESLAGER. *The Stamp Act Congress*. Newark: University of Delaware Press. 1976. Pp. 279. \$14.50.

The Stamp Act Congress of 1765 was the first intercolonial assembly in the long imperial crisis that culminated in American independence. The

first published study of such an event naturally raises the expectation that it will be an important book, or at the very least, a useful book. On both counts C. A. Weslager's *The Stamp Act Congress* will probably disappoint those interested in the coming of the American Revolution. Weslager's study offers almost no original interpretation of this long-neglected convention. This is especially unfortunate because the congress offers the historian a rare opportunity to explore the nature and degree of the thirteen colonies' capacity for unity at an early stage in the development of the American nation.

Weslager begins his study with a few well-known facts about George Grenville's imperial reforms and then describes the methods of imprinting and selling the paper required under the Stamp Act. Succeeding chapters portray each member of the congress and discuss its deliberations, but add little to what is readily available in several local studies and in Edmund Morgan's much shorter, more clearly focused account in *The Stamp Act Crisis*. In the fourth chapter, Weslager presents his monograph's principal reason for existence: the first publication of the congress' full Journal. When the delegates' often-published resolves and addresses to the King, Lords, and Commons are removed from this Journal, however, the remainder is too brief and unrevealing to be of much value.

Only in his final chapter does Weslager advance an original thesis: that the Stamp Act Congress delegates, in particular those who were merchants, prompted New York's mercantile community to initiate the colonies' first boycott against British goods. Unfortunately the evidence presented to support this contention is entirely circumstantial; the author has apparently done almost no research on New York commerce and politics in the 1760s to confirm it. The evidence presented in Weslager's own second chapter shows that most delegates were not merchants; the only major exception was New York's delegation, which suggests that the boycott probably had a local genesis.

Finally, in a work whose principal value is its collection of heretofore scattered information, careless errors—such as frequent references to "Lord" George Grenville, the appearance of "Jonathan Trimble," later governor of Connecticut (p. 68), the statement that Eliphalet Dyer was a graduate of "Yale law school" (p. 69), and the contention that in 1765 Pennsylvania had 300,000 inhabitants and was the second most populous colony in British North America (pp. 84–85)—are particularly regrettable.

RICHARD ALAN RYERSON
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PHILIP S. FONER. *Labor and the American Revolution*. Westport, Conn.: Greenwood Press. 1977. Pp. xi + 256. \$14.95.

"And now it is the people's turn," Richard B. Morris said in his 1976 AHA presidential address. He was referring to the increasing attention being given to the history of the common man during the Revolutionary era. For nearly two decades, a group of scholars—loosely and somewhat erroneously referred to as the "New Left"—have been digging in a rich mine of common man's history, left unattended since the Consensus historians demolished the views of Carl Becker and the Progressives. What they have uncovered does not have the unalloyed purity and simplicity of the class conflict once supposed to have characterized the struggle for "who should rule at home," but class-related social and economic ferment are nevertheless its basic elements. Focusing on persons and groups heretofore written off as "inarticulate," historians have discovered that the Revolutionary era provided means to challenge old authorities, to alter traditional deferential social and political habits, to voice grievances which were peculiar to men and women of meaner station. Because these historians have been breaking new scholarly ground, their work has necessarily been piecemeal—studies of single cities, of subgroups of the lower classes, of individuals, or of minor episodes and crises. The time has come for synthesis, and that is what Philip S. Foner attempts in this excellent book.

The subject is the urban common man—the mechanic class—during the 1760s and 1770s. Those who have read the recent literature on this subject closely will find little that is new in Foner's book, but neither will they find much in the works of others that he has overlooked. His object is to stitch together the materials others have found and the conclusions they have reached, to find interrelations and general patterns. If the relationships sometimes seem strained, it is only because Foner must connect scholarship which is uneven in quality, various in intent and subject-matter, and most of all incomplete. There remain large gaps in our understanding of the common man's Revolution, notwithstanding the legitimacy Morris has recently given the subject, and Foner must gloss over them. Yet this study deals sensibly with the soft spots: it suggests where scholarship is weak (for that reason this book would be a good beginning point for a new student on the Revolution), and it draws generalizations only when they are warranted.

At times Foner may claim too much for the viewpoint he represents. It may be a little myopic to suggest that it was principally the urban me-

chanics "who pushed the struggle forward during the ten-year period [before 1776] and provided the apparatus and manpower for the transition from resistance to Independence" (p. 45). And at times one wishes Foner would release himself from the familiar political history of the Revolution and speak more directly about the social underpinnings of the mechanics' participation in those events—a subject about which we know more than Foner suggests. Finally, it is disappointing that this book restricts its scope to the period before Independence was secured, for there is a wealth of material on the common man's Revolution for the years after Independence, a period which logically should be considered with the materials scrutinized in this volume. These matters will have to await a second synthesis, which will be possible only after many more particularistic studies have advanced our knowledge beyond the point summarized in this book. Despite these shortcomings, Foner's study is exceedingly important. Those who teach the history of the American Revolution will find it a fresh and challenging monograph for their students to read; those who study the Revolution will find it a useful reference point from which to begin new research.

CHARLES S. OLTON
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MICHAEL GLOVER. *General Burgoyne in Canada and America: Scapegoat for a System*. London: Gordon and Cremonesi; distributed by Atheneum, New York. 1976. Pp. 254. \$19.95.

As the title suggests, this is an apology for General John Burgoyne's performance in the War for American Independence. Burgoyne, who played only minor parts in the opening campaigns of the war, had a principal share in planning and conducting the campaign of 1777. In February of 1777, while home on leave, Burgoyne proposed that an army be sent south from Canada to Albany, to join or cooperate with another British army from New York in dividing and conquering the colonies. This plan was sound enough, but Burgoyne alone was faithful in carrying it out. As commander of the Canadian army, he soon found that he did not have all of the troops that had been promised; and after making rapid progress to the Hudson, he learned that there was no army from New York to join or cooperate with him. Refusing to believe that Howe would fail to cooperate, Burgoyne persisted in going to Albany lest he violate his orders and ruin the general plan of campaign. In October, without supplies from Canada or direct support from New York, he surrendered at Saratoga. Even so, Glover contends, Saratoga would not have been a decisive defeat had the rebels honored

the terms that Burgoyne arranged; and Burgoyne would not have been blamed for losing his army had Howe been less influential.

Such arguments are scarcely convincing. To be sure, Germain, Howe, the Americans, and chance all contributed to Burgoyne's surrender. But so did Burgoyne, and no amount of pleading is likely to persuade historians that Burgoyne had not the authority to interrupt his march on Albany. Nor will students of the Revolution readily agree that Burgoyne's surrender was decisive only because his army was detained in America. Glover's arguments are, of course, no more partial than his research. He has relied all too often on Burgoyne's accounts; and he has ignored such important sources as the Clinton Papers at the Clements Library, the official correspondence at the Public Record Office, Gerald Brown's *American Secretary*, and Paul Smith's *Loyalists and Redcoats*. In short, this is not a definitive biography.

IRA D. GRUBER
Rice University

LARRY G. BOWMAN, *Captive Americans: Prisoners during the American Revolution*. Athens: Ohio University Press, 1977. Pp. viii, 146. Cloth \$9.00, paper \$3.50.

Captive Americans is mistitled. The work focuses primarily on British prisoner-of-war policy and on the American government's response to that policy. Larry G. Bowman faults the British performance in a number of instances but emphasizes that "British policy could not be typified as cruel" (p. 133). This general thesis is sound, but the history of some prisons, such as the prison ships in New York, demonstrates that at times the British were most cruel.

Surprisingly, *Captive Americans* proves least satisfying as a study of captive Americans. What the author offers, with few exceptions, is a history of prisoners without the prisoners. Part of this is by design. Bowman promises to avoid quoting prisoners, and he is true to his word. By following this course Bowman manages the difficult task of draining the human drama out of "the emotional trauma" (p. 4) of being a prisoner. Worse yet, he slides over or simply ignores day-to-day prison life and the response of the prisoners to their lot. For example, Bowman claims we cannot discover how many prisoners escaped or defected and takes this as license to offer only the most cursory analysis of why people tried to escape or chose to defect. Yet some reliable statistics on escapes and defections are presented in works Bowman cites. Valuable statistics can also be found in manuscript sources housed in England, but Bowman only examined sources available in the United States. He even fails to cite the statistics or to consider the pro-

vocative conclusions drawn from them and other material by Jesse Lemisch in his well-known essay on "Jack Tar in the Streets." Indeed, much that is already known about American prisoners does not find its way into this work. Thus, Bowman's work on the prisoners themselves is not even a good summary of the available secondary sources.

In short, the general reader will find *Captive Americans* less than captivating, and historians interested in American prisoners of war during the Revolution will find the work of little value.

JOHN K. ALEXANDER
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ESMOND WRIGHT, editor, *Red, White and True Blue: The Loyalists in the Revolution*. New York: AMS Press, for the Institute of United States Studies, 1976. Pp. ii, 222. \$10.50.

It is perhaps the nature of the beast that volumes created out of symposia are uneven in quality. This collection, a product of the 1975 Conference on American Loyalists, sponsored by the Institute of United States Studies, University of London, is no exception. What is more surprising, however, is the apparent transatlantic knowledge lag: if the America of 1775 was unable to keep up with cultural or intellectual currents at home, in 1975 English students of the Revolution seem to be curiously behind in their scholarship. In an opening essay, for example, author Ann Gorman Condon argues the need to revise current notions that the loyalists lacked "a clear cut, positive . . . political philosophy." She then goes on to prove, through a case study of Jonathan Sewall of Massachusetts, that loyalists did embrace such a philosophy, based on their definitions of empire and liberty. True enough. But Sewall has been used to prove such a point before (in my own book, 1974), and Robert Calhoon has made the same argument, more fully and subtly, in his 1973 study of the *Loyalists in Revolutionary America*. Similarly, old paths are retrod in K. G. Davies' essay on British efforts to restore civil governments during the war, for John Shy's astute military pieces make similar points in a richer context.

The volume does offer a sampling of the use of new sources for loyalist history. L. F. S. Upton gives us the British in America, counterpart to Mary Beth Norton's *Americans in Britain*, in an essay that looks at the problems Claims Commissioner John Anstey faced in attempting to validate, on the American scene, claims made by exiles in England. But the essay seems to represent "work in progress," for a sense of undigested material and unexplored implications pervades the piece. In the same vein, G. A. Rawlyk gives us a mini-biography of the Reverend John Stuart. Rawlyk's

sources are not entirely unfamiliar—Barbara Graymont culled them in 1972—but this essay's concentration on the missionary's own perspective rather than the Mohawks' yields some interesting results. Still, the piece is too short and tentative to exhaust the author's material or his ideas.

Two fine essays brighten the book. The first, by Ian Christie, reviews with clarity and authority the successive ministerial perspectives on the needs of the Empire and the resulting policies. Wallace Brown's comprehensive survey of the impact of Loyalist emigration upon the West Indies is also a rewarding essay. Brown's look at the processes of acculturation, of absorption, and of what might be called today cultural separatism raises many parallels for the study of both nineteenth-century American immigration history and the twentieth-century phenomena of refugee camps and population relocations.

In the end, *Red, White and True Blue* is far less snappy than its title. Perhaps the light-hearted touch on the cover is a clue that the essays celebrate a gathering of the clan more than anything else.

CAROL BERKIN
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ELLEN GIBSON WILSON. *The Loyal Blacks*. New York: G. P. Putnam's Sons. 1976. Pp. xi, 463. \$10.95.

JAMES W. ST. G. WALKER. *The Black Loyalists: The Search for a Promised Land in Nova Scotia and Sierra Leone, 1783-1870*. (Dalhousie African Studies Series.) New York: Africana Publishing Company. 1976. Pp. xvi, 438. Cloth \$24.00, paper \$10.00.

Hardly anyone needs to be reminded that, despite the euphoric feeling of national togetherness which characterized the celebration of the United States' bicentennial in 1976, two centuries ago the white colonial population of British North America was deeply divided in its attitude toward the independence movement. In contrast, the enslaved black population in colonial society was overwhelmingly united in desiring a loyalist victory over the rebel cause. This unity derived from very practical considerations. Early in the conflict the British attempted to attract the rebel-owned slave population to the loyalist side by promising security and freedom to all blacks who deserted their masters. Little if any humanitarian sentiment governed this move; its intent was to injure the rebel cause by removing its major source of labor. As a consequence, a large number of slaves made their way to the British side, lured by the prospect of a new and better life at the conclusion of the conflict. Ellen Gibson Wilson and James W. St. G. Walker both focus on the history of some of these "black

loyalists": initially on the few thousand who settled in Nova Scotia after the British defeat and, subsequently, on the smaller number among these "Nova Scotians" who departed from the Canadian maritime peninsula in 1792, eventually settling in Sierra Leone as black American immigrants on African soil.

The two books treat essentially the same subject matter. Both describe how Nova Scotia failed to fulfill the expectations of most of the blacks who were brought there; how the black loyalists were victimized time and again by racial discrimination, threatened with re-enslavement, unable to obtain promised lands from provincial officials, and exploited shamelessly as cheap labor; how, in return for a pledge of land and liberty in Sierra Leone, nearly twelve hundred of them embarked for West Africa to become settlers—participating in the largest migration of free blacks in history. Both detail the disenchantment of these Nova Scotians with their new home where, frustrated again by unfulfilled promises and fearing attack from neighboring Africans and abduction by slave-traders, they progressively compromised their hopes for self-sufficiency in favor of the security offered by the Sierra Leone Company. Both also demonstrate extremely well how, from adversity and the multitude of shared experiences in America and Africa, a distinctive culture marked by a powerful sense of separate identity emerged among the black settlers.

Given such overlap, what is impressive about the books is their difference in scope and execution. Wilson ends her account of the Sierra Leone settlement early in the nineteenth century, when control over its affairs passed from the Sierra Leone Company to the British Government, and the American blacks lost their limited right to elect representatives and maintain a voice in their own rule. In this respect she might be criticized for breaking off her story at too early a date: a cutoff in the 1840s, some fifty years after the Nova Scotians arrived in Africa, would have provided a fuller understanding of their impact on Sierra Leone and subsequent fate. Walker extends the Nova Scotians' story to 1870, examining the "further evolution" of their society after they were joined in Sierra Leone by the Jamaican Maroons and, following the abolition of the slave trade, by thousands of Africans "liberated" from slave vessels. Unlike Wilson, he also returns to the black Loyalists who remained behind in America after 1792 in an epilogue which, although brief and impressionistic, does in his case justify the book's title.

While Walker's range is greater, he is generally less successful than Wilson in reconstructing the "voice" of the black settlers and thus portraying a full sense of their lives. Wilson's talented use of

contemporary quotations and frequent focus on individual participants convincingly captures the human dimension and flavor of the times. She provides detail without tedium, vividly recreating events—the voyage from America to Africa, the harshness of life in the infant colony—that help us understand and appreciate the resilience and adaptability which characterized the black settlers and allowed them to survive the ordeal of transplantation.

Walker, moreover, overemphasizes the Nova Scotians' importance as a cultural model to be emulated by other black immigrant groups who followed them into Sierra Leone. The religion, architecture, food, dress, ideas of land tenure, and zeal for education of the transplanted black loyalists did undoubtedly influence the culture and society of the Africans who were freed in large numbers in Freetown after 1807. But the Nova Scotians were few in number and, as Walker himself acknowledges, declined both absolutely and as a proportion of the total population of the colony. By 1851 there were 112 persons identified as Nova Scotians left in the colony and by 1862 only 22. Consequently, the dominant "model" was a dynamic one all along—as different in 1830 from its 1800 incarnation as was the 1800 one from the 1792 version, with infusions from African and European sources as well as American. Ultimately, what came to be known as Sierra Leone Creole society gained its strength and cultural vitality not from a single spring but from a heterogeneous fount.

LEO SPITZER
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MORTON J. HORWITZ. *The Transformation of American Law, 1780–1860*. Cambridge, Mass.: Harvard University Press. 1977. Pp. xvii, 356. \$16.50.

This book is a major contribution to American history. Morton J. Horwitz argues that a fundamental shift took place in American private law during the period from the Revolution to the Civil War—a movement he traces in torts, contract, property, and commercial law. Eighteenth-century common law operated from accepted rules which were thought to embody timeless principles of equity and natural law. Legislatures "made" law, but judges were presumed only to "find" it and, once found, to maintain it via *stare decisis*. With the growth of the individualistic, competitive, market-oriented society of the nineteenth century, the common law as it was made by state judges became openly policy oriented. Concern for principle and morality declined along with deference to legislative law-making as the courts shaped law to facilitate the exchange of goods and maximize the productivity of property. No longer

would the law bestow the right of absolute ownership on property, if doing so would hold back economic growth. Contract law accommodated the new age by dropping considerations of fairness and equity for the "will theory," wherein the sole consideration was agreement between parties forewarned by *caveat emptor*. The substitution of damage judgments for orders of specific performance of the terms of the contract advanced the market economy by transforming contractual obligations into money value. The demise of usury law and the growth of bankruptcy law, the shift from nuisance to negligence, as well as doctrinal changes in insurance and workmen's liability facilitated economic growth by releasing capitalists from the social costs of their activities. Legal formalism, talk of legal science, obscured the policy bias of the common law. But in fact, Horwitz finds, the bench and bar put themselves in a working relationship with the dynamic segments of the business community to the detriment of static capital and the less efficient and less powerful segments of society.

Horwitz's argument, though often complex and technical, is lucidly presented, prodigiously researched and sustained throughout by a brilliant analytical ability. The resulting contribution for both legal historians and general students of antebellum society is enormous. The delineation of the doctrinal transformation of private law, in itself, is a major accomplishment, since most of the areas discussed have never been treated conceptually or historically. On the interpretive level, the book modifies further the state political-economic studies of Louis Hartz and Oscar and Mary Handlin by showing that, however much antebellum state legislatures withdrew from economic intervention, the states through their judicial systems continued to make economic policy of a most fundamental nature. Indirectly, the book challenges the consensus-continuity historians who write intellectual history without reference to the social groups who articulated ideas and benefited from them, historians of social change who overlook the impact of that change on human beings, and those who gloss over power relationships between social groups in favor of abstract statistical measurements of them.

As important books do, this one also poses new areas for investigation. Legal historians, for example, will want to know whether the general trend toward legal instrumentalism was punctuated by strong regional and state variations, especially in the agrarian South. A study of the common law of a Southern state (patterned after William Nelson's superb Massachusetts study) might in fact be the most conclusive way to answer that perplexing question of whether the old South was agrarian capitalist or preindustrial. The brief chapter entitled "The Relation between the Bar

and Commercial Interests" whets our appetite to know more about the complex interaction of these two powerful groups. Clearly, our understanding of the antebellum legal profession will be incomplete until more attention is paid to the impact of legal change on patterns of practice and legal education. Also we need to know more about the complex interaction between economic development and legal ideas—a theme which runs through the book, but which is not fully developed. Indeed, the powerful impact which the national market system exercised on American law suggests the need to explore the impact of that system on other areas of American culture. In short, American history has much to learn from American law. Historians should be grateful to Horwitz for building such a powerful bridge between the two.

KENT NEWMYER
University of Connecticut,
Storrs

DONALD M. JACOBS, editor. *Antebellum Black Newspapers*. Westport, Conn.: Greenwood Press. 1976. Pp. xii, 587. \$35.00.

GRACE D. PARCH, editor. *Directory of Newspaper Libraries in the U.S. and Canada*. New York: Special Libraries Association. 1976. Pp. xii, 319. \$9.75.

LUBOMYR R. WYNAR and ANNA T. WYNAR, editors. *Encyclopedic Directory of Ethnic Newspapers and Periodicals in the United States*. 2d ed. Littleton, Colo.: Libraries Unlimited. 1976. Pp. 248. \$15.00.

Serious journalism history is a relatively new phenomenon. Until recently, only a few distinguished researchers had devoted more than passing attention to the field, but each of those has made substantial contributions. Isaiah Thomas, himself a prominent editor of the revolutionary era, was the first; since then, the field has been dominated by a tireless few—Frederick Hudson, Willard Bleyer, Frank Luther Mott, Edwin Emery. For each the work has been slow and painful, for there was little if any body of accumulated source materials. Bibliographies were almost nonexistent. Clarence Brigham's monumental effort in cataloguing American newspapers up to the year 1820 has not yet been duplicated.

Slowly, however, the gaps in basic source materials in press history are being filled. Happily, some fundamental compilations are now appearing not only about the mainstreams of the American press but also in those areas that have been to a large extent ignored by the trail-blazing historians. In recent years, collections of source materials on women in journalism have begun to appear as well as various works on regional press history. Mott, Emery, and other standard press historians have tended to place disproportionate

weight on the eastern press, especially in New York City.

Particularly welcome now is the expanded edition of Wynar's *Encyclopedic Directory of Ethnic Newspapers and Periodicals in the United States*. This work is perhaps the most extensive bibliography of an element of the American press usually given only passing attention by scholars. A 1975-76 survey by the Wynars found 977 ethnic publications with a total circulation of over nine million. The bibliography provides general information about each of those publications. Historians will find addresses, telephone numbers, and names of editors useful as sources for further investigation. Lamentably, however, the bibliography supplies no information about the history of any of the publications. We still await a significant updating of Robert Park's 1922 study, *The Immigrant Press and Its Control*. The Wynars' bibliography, however, should be a useful instrument for those who would explore the role the ethnic press has played in American press history and indeed how that role has been changing as various nationality groups have been assimilated.

Another useful addition to the store of source materials is Grace Parch's *Directory of Newspaper Libraries in the U. S. and Canada*. Parch, who is library director of the Cleveland *Plain Dealer*, has of necessity compiled an incomplete directory, since she can list only those newspapers which responded to her inquiries. Information is available, however, on the extent of the holdings of most major newspaper libraries, together with services regularly available to historians and other members of the public by those libraries. The New York *Times*, for instance, reports its holdings include 1,200 drawers of clippings and 614 drawers of photos and art work. By contrast, the Butler *Eagle*, of Butler, Pa., reports 35 file drawers, and the *Deseret News* of Salt Lake City, Utah, 80 drawers. This worthy effort commissioned by the Newspaper Division of Special Libraries Association, is a first step in what is expected to be an ongoing project. The present directory includes files on the holdings of the libraries of 273 U. S. newspapers and 22 Canadian newspapers.

An even more striking and immensely useful work is the bibliography edited by Donald M. Jacobs, in which he catalogues and in fact provides brief descriptions of the contents of every article published in the first black newspaper in the United States, *Freedom's Journal*, as well as those in three other early representatives of the black press, *The Rights of All*, *The Weekly Advocate*, and *The Colored American*, all published in the fifteen-year period between 1827 and 1841. Historians interested in the treatment of political, social, and other issues by black newspapers in those years will, for

example, find a tantalizing collection of information in Jacobs' eight pages of items from *Freedom's Journal* listed under the heading of slavery. An article on November 7, 1828, for instance, "describes the terrible conditions on a slave ship going from Baltimore to New Orleans," and an article on July 13, 1827 "relates the history of Hebrew slavery and compares it with modern slavery." Jacobs' painstaking work will make it easier for those who come after to explore the American scene from the perspective of black editors and to help fill in some of the largest gaps in American social history.

HERBERT ALTSCHULL
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ALEXANDER DECONDE. *This Affair of Louisiana*. New York: Charles Scribner's Sons, 1976. Pp. x, 325. \$12.50.

The acquisition of the vast mid-continental region of the United States from France in 1803—the Louisiana Purchase—is an historical fact embedded in the minds of all Americans. It is rather surprising, therefore, that no professional historian has scrutinized the subject in a book-length account since World War II. So Alexander DeConde's synthesis, which covers the background, the negotiations, and the immediate aftermath of *This Affair of Louisiana*, fills a void in American historiography.

As DeConde admits, much that he tells is familiar: the American representative to France, Robert R. Livingston, wanted sole credit for acquiring Louisiana and was extremely upset when special envoy James Monroe arrived in France to lend a hand. Meanwhile, France's inability to crush the Haitian insurgents of Toussaint L'Ouverture, coupled with the frightful impact of tropical diseases on General Charles Victor Emmanuel Leclerc's army, doomed Napoleon's dream of a New World empire. The First Consul told his brothers, Joseph and Lucien Bonaparte, about his intention to sell all of Louisiana while he was bathing in the Tuileries. The great historian Henry Adams revealed all these things almost nine decades ago.

On the other hand, DeConde provides interesting and always informative insights into the transaction. For example, he downplays the theme of America's advantage from Europe's distress in gaining the territory, and he downgrades the role of frontiersmen in exerting pressure on Spain and France, making it easier for the United States to obtain the land. To the author, the acquisition was "the result of a conscious expansionism of an imperial creed promoting action" (pp. ix-x). DeConde reiterates this idea in his last and perhaps most interesting chapter, "The Imperial Thrust."

Judgments are not lacking. France had no right to sell Louisiana, as the Spanish protested, and as Napoleon and Jefferson knew. "This knowledge did not stop their cooperation in despoiling Spain's New World empire, but it did prevent the American government from assuming the role of an innocent purchaser" (p. 194). Similarly, DeConde denies that West Florida was included in the bargain, notwithstanding Jefferson's claims then and the verdicts of various historians since.

The only weakness in DeConde's book is his failure to treat the role of land speculators adequately. Some evidence exists that a ring of Gallo-Americans (as John Randolph called them) were involved in the maneuvering that preceded the sale of the Louisiana region. Furthermore, land speculation may have motivated minister Livingston in his negotiations for Louisiana as much as a desire to obtain eternal glory in American history. Finally, there is the interesting fact that Fulwar Skipwith, who was linked with Jefferson's drive for West Florida, emerged as the leader of the short-lived Republic of West Florida, created by a revolt in 1810.

In summary, *This Affair of Louisiana* is not the definitive treatment of the subject. It is, rather, a thoughtful, well-written synthesis which places the Louisiana Purchase in its proper historical context; it should make interesting reading for a wide audience.

CLIFFORD L. EGAN
University of Houston

PAUL RUSSELL CUTRIGHT. *A History of the Lewis and Clark Journals*. Norman: University of Oklahoma Press, 1976. Pp. xxi, 311. \$17.50.

When you achieve immortality you can expect to be traduced and trivialized as well as idolized. Lewis and Clark loom on the western horizon like gods, and there seems to be no end to the outpouring of trail-tracings and rehashes of their exploits. In contrast to this trivia, Paul Russell Cutright's new book is an important contribution, one that should close the select circle of major scholarly works on the subject.

We don't often get histories of histories, and rarely do historians themselves emerge as heroes. Here "the plot" is the publishing history of the Lewis and Clark journals, field notes, and letters, and the heroes are the successive editors. The gentlemen so honored are Nicholas Biddle, Elliott Coues, Reuben Gold Thwaites, Milo Milton Quaife, Ernest Staples Osgood, and Donald Jackson. The net gain for the reader, after decades of murkiness on the subject, is clarification of the known literature of the Great Expedition, the journals of Gass, Ordway, Whitehouse, and other subordi-

nates as well as those of the two incomparable captains. The precarious career of the documents is itself an epic of scholarly struggle, triumph and defeat, a fitting sequel to the grand geographical adventure.

Cutright stresses Thomas Jefferson's role in requiring detailed records and his concern for their preservation and publication. The respective journalistic achievements and lapses of the two captains are analyzed. Cutright identifies myths concerning Lewis and Clark which should be exterminated, and he examines their stature as pioneer naturalists. He brings to his task an enthusiasm stemming from his own previous work as a biologist who marveled at the scientific accomplishments of these explorers despite incredible handicaps (*Lewis and Clark: Pioneering Naturalists*, [1969]).

As a bonus the author identifies writers and artists, other than the editors, who have attempted interpretations of various aspects of the Lewis and Clark saga. There are two serious omissions here. I refer first to a magnificent painting made on site by the late great Thomas Hart Benton, entitled "Lewis and Clark at Eagle Creek," which I would rank with anything by Remington or Russell. And no discussion of this sort should omit the authoritative volume on physical aspects of the expedition, and historic sites associated with it, written by Roy Appleman (*Lewis and Clark*, [1975]).

MERRILL J. MATTES
Denver, Colorado

BASIL DMYTRYSHYN and E. A. P. CROWNHART-VAUGHN editors and translators. *Colonial Russian America: Kyrill T. Khlebnikov's Reports, 1817-1832*. Portland: Oregon Historical Society. 1976. Pp. viii, 158. Cloth \$12.00, paper \$6.50.

Two of the six parts of Kyrill T. Khlebnikov's manuscripts, "*Zapiski o Amerike*," were published in St. Petersburg in 1861 and have since served as an important source for the history of Russian America, of Hawaii, Alaska, and California. Hubert Bancroft, Semen Okun, Richard Pierce, Anatole Mazour, Ekkehard Völkl, Janus Gibson, Nikolai Bolkhovitinov, Stuart Tompkins all use Khlebnikov's writings. The two parts published in 1861 have now been translated, and the editors promise us the publication of the remaining four.

Khlebnikov (d. 1838) was for fifteen years (1817-32) administrator for the Russian-American Company and second in command in New Arkhangel (Sitka). His manuscript on New Arkhangel (part 1) and on Fort Ross (part 2), constituting his report to the directors of the company, contains a wealth of information; his power of observation, clarity of exposition, and factual presentation furnish us with a unique picture. Surveying the his-

tory and geography of Russian-America since 1795, he describes the native populations and their customs, their living conditions, hunting methods, and their relations to Russian colonists. He lists animals and plants and, naturally, deals at length with the sea otter and its hunt. He deals with the life of the employees of the company, their working and living conditions, their habits, income and debts, their handicrafts and trade, their ship-building and other activities including education and organizational problems. Especially extensive is his information on economic matters. Not only does Khlebnikov describe them, but he also gives an abundance of statistics on trade, inventories, values, prices, livestock, etc., and even furnishes balance sheets of the company's operations.

The editors have added numerous well-chosen illustrations and reproductions of contemporary maps, and a concise bibliography. But the translation is not always felicitous. Most of all, the student of Khlebnikov's report will miss a careful and extensive scholarly apparatus. A unique chance, which would have enormously increased the value of the undertaking, has been passed over—no doubt after due consideration by the editors. Although the reader will regret the failure to invest this additional effort in so worthwhile a project and to provide us with a careful commentary, he will nevertheless be grateful for what he gets.

WALTHER KIRCHNER
Princeton, New Jersey

PETER C. MARZIO. *The Art Crusade: An Analysis of American Drawing Manuals, 1820-1860*. (Smithsonian Studies in History and Technology, number 34.) Washington, D.C.: Smithsonian Institution Press. 1976. Pp. viii, 94.

As one facet of early-nineteenth-century cultural nationalism, many Americans were concerned with bringing a knowledge of art to the people and thereby, it was hoped, improving national taste and elevating morals. Between 1820 and 1860 more than 145 drawing manuals were published in the United States. Most of them set forth a body of related ideas concerning art and its place in American life which constituted what Peter C. Marzio views as an "art crusade." The efforts of the leading figures of this rather amorphous movement—such painters and draughtsmen as John Rubens Smith, Rembrandt Peale, and John Gadsby Chapman—were directed toward adapting art traditions of the past to a democratic society. The attitude of many citizens that drawing was a frivolous pastime, suitable only for women and children, provided a major obstacle to these endeavors, but even so there was, Marzio finds, widespread ac-

ceptance of what the "Crusaders" were attempting to do.

A democratic art had to be straightforward and easily understandable, and the methods followed in the drawing manuals seemed to meet these requirements. The books set forth a clearly structured system of drawing based on the concept that "lines were the essence of form." The esthetic theories of Sir Joshua Reynolds were commonly followed as guidelines, while the educational views of Johann Pestalozzi served to make drawing into a part of a general pedagogical system. The approach of the manuals, coming from the past, tended to be rigid and mechanical and by and large at odds with the romanticism of the period. No wonder, then, that by 1860, the art crusade fell apart, as new Ruskinian ideas came to the fore, rejecting the authority of the past and focusing on light and shade rather than on line.

Marzio's study includes ample illustrations, providing good examples of the techniques of instruction in the manuals, and he explicates well the cultural implications of the books he considers. As a guide to the reader, he also includes a systematic list of American drawing books published by or for American authors between 1820 and 1860. The monograph is well written and informative.

PAUL R. BAKER
New York University

LEONARD J. ARRINGTON *et al.* *Building the City of God: Community and Cooperation among the Mormons*. Salt Lake City: Deseret Book Company. 1976. Pp. xiii, 497. \$7.95.

"Why we could organize millions into a family under the order of Enoch," declared Brigham Young in 1873. Young was speaking for the quasi-communist United Order of Enoch (or the "Law of Consecration and Stewardship"), as revealed by God to Joseph Smith, the Mormon Prophet, in Kirtland, Ohio in February 1831. The Order had just begun to make progress in Zion (Independence, Missouri) when the Saints were driven out in 1833. But in various guises the Mormon communal tradition has persisted to this day.

This book is itself a communal effort headed by Leonard J. Arrington, one of the most important and prolific contributors to Mormon studies. The first Ph.D. ever to attain the high Mormon office of Church Historian, Arrington, aided by a large staff of trained historians and archivists, has been applying his impressive administrative and historical gifts to the scholarly exploitation of the vast resources of the Church Historian's Office; and this book is only one of several recent accomplishments.

Except for the outstanding chapter on Kanab's

unlucky Order and new details on other topics, there seems, at first glance, to be little new here. The authors trace the early history of the order and then move on to Utah with excellent summaries of the unsuccessful revival of the Law of Consecration (1834-38) and the Church's very successful commercial cooperatives. Between 1868 and 1880 the Saints founded no less than 148 cooperative stores throughout Utah. These retail and manufacturing cooperatives helped prepare the way for the revival of the radically egalitarian United Order, under which the Saints would live as members of a commune of labor and not just as individual stockholders in a cooperative business enterprise.

Despite the familiarity of much of their material, the authors, in weaving together diverse studies in narrative form and in supplying additional information, have given us the first overall view of Mormon communitarian history. This enables them to make comparisons among the various local cooperatives and United Order communes, to explain variations among them, and to suggest reasons for decline. For example, even at its height Mormon communitarianism in Utah suffered from the conflict between Brigham Young's idealistic desire to restore the "gospel order of equality" of 1831 and the materialistic realities of the 1860s and 1870s, between his goal of United Order life as a "well regulated family" and the desire of others to make do with cooperatives. Moreover, the authors clearly show that the enforcement in 1884 of the Edmunds Anti-polygamy Act destroyed not only polygamy, but also the old leadership which had sustained the communitarian and cooperative institutions.

The book has some weaknesses: in documentation (e.g., for the Shakerism of the Pratt family); omissions (an egregious one like ignoring Nauvoo and tantalizing ones like the unexplained "consecration" of a daughter by her father—his gift of her to the Church as property); and contradictions (at one point Brigham Young advocates United Order equality, at another rejects it). One could also argue against a subtle minimizing of non-divine influences on Smith's revelations, like his knowledge of Shakerism. But these flaws do not lessen the importance of the book.

The authors conclude that despite the extremely conservative capitalist views of Mormon leaders since the 1890s, the Saints still practice some communalism in their well-known church Welfare Plan; that the Saints still believe in the millennium; and that given some great social or economic catastrophe, we may yet witness a "sudden renewal of Mormon communitarianism."

MARIO S. DE PILLIS
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Amherst

WILLIAM WISE. *Massacre at Mountain Meadows: An American Legend and a Monumental Crime*. New York: Thomas Y. Crowell Company. 1976. Pp. 317. \$11.95.

This is a history of one of the most tragic events in the opening of the West. In response to bitter political discord a federal army of occupation approached Utah in September 1857. Recalling their expulsion from the Midwest, Mormons in southern Utah over-reacted and encouraged Indians to attack an emigrant party of some 120 people. When the emigrants repelled the Indians, Mormons joined the Indians to kill the entire party, except sixteen or seventeen children. Mormons laid the crime to the Indians and assumed a determined silence which was penetrated, but not broken, by the 1876 trial of John D. Lee, one of the principal participants. The nation was outraged and the Mountain Meadows Massacre became a festering issue in a period of conflict which extended to 1890. Even thereafter its memory was a suppressed blight among Mormons. Finally, in 1950 a native daughter, Jaunita Brooks, gave the tragedy its definitive study in *The Mountain Meadows Massacre*.

In this new book, William Wise tells the massacre's story in thirty-five short chapters. The first two introduce the victims, the Fancher Train. The next twenty-two chapters set the stage by recounting a list of Mormon atrocities beginning with the church's founding. Chapters twenty-four through thirty-two recount the details of the massacre, while the remaining chapters consider the events that followed. Wise knows what he wants to say and says it in a fast-moving and clear style.

This book is unfortunate in many respects. It harks back in style and objectives to the sensational literature of the nineteenth century. Little or no new evidence is presented. New interpretations are lacking. Most distressing, however, is the fact that it restates an old indictment rather than illuminating the past.

Wise's use of sources is also unfortunate. Recent scholarship is largely ignored. Exceptions are Stanley Hirshon, *The Lion of the Lord*, Fawn M. Brodie, *No Man Knows My History*, and Juanita Brooks, *The Mountain Meadows Massacre*, which, however, is drastically discounted as an effort to "defend the Church's reputation at any cost." Use of primary sources is limited. Holdings in the Mormon church archives are ignored. When primary sources are used it is frequently in a way that suggests that the author cited them from secondary sources. Perhaps the most startling example of this was Wise's quotation of Hirshon, *The Lion of the Lord* for the few citations he takes from *A Mormon Chronicle: The Diaries of John D. Lee, 1848-1876*,

long available in published form, but otherwise avoided in his study. On the other hand, *The Life and Confessions of John D. Lee*, dictated after Lee's trial, comes near being the documentary mainstay of the work.

Wise closes his account with the hope that when "the people of the Great Basin" learn that no group has sole claim upon the truth they will perhaps be able to make peace with their past. Although it is apparently lost upon the author, few serious readers will fail to see the irony of this conclusion to a book that chooses a sensationalized single-cause approach to an extraordinarily complex event.

CHARLES S. PETERSON
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LEONARD J. ARRINGTON. *From Quaker to Latter-Day Saint: Bishop Edwin D. Woolley*. Salt Lake City: Deseret Book Company. 1976. Pp. xiii, 592. \$6.95.

The attempt to bridge the gap between antiquarian family history and serious historical biography raises special problems, many of which are illustrated in this study. Commissioned by a descendant of Edwin D. Woolley and written by Mormon Church Historian Leonard J. Arrington, this biography recounts the life of a middle-rank nineteenth-century Mormon leader who was intimately associated with the effort to build the Mormon kingdom, both in Nauvoo, Illinois, and in early Utah. Woolley's life and activities reflect the diverse roles played by many Mormon pioneer figures; he was variously a business promoter, missionary, legislator, polygamist, ward bishop, and loyal associate of Brigham Young.

Faced with the difficult task of creating a meaningful narrative when Woolley's own diaries and records are so pedestrian, this biography carefully draws upon additional sources to reconstruct the people, places, and events of his life. The result, despite the best of intentions, often appears to be essentially an expanded historical chronology, even when the original documents have intrinsic color of their own. For example, the study quotes the complete text of a moving personal letter from Woolley's devoted first wife, who wrote after temporarily leaving him a year after he had adopted polygamy. Rather than analyzing the complex emotional reactions expressed in the letter or comparing her reactions to those of other individuals struggling to come to terms with polygamy, the narrative immediately moves on to the external political crisis in Nauvoo. In this as in other instances, information is presented and a larger context is ably described, but the interpretive linkages remain only implicit or partially developed. Arrington's previous studies have shown him to be an

accomplished interpreter of Mormon economic and social life, with an outstanding ability to relate individual experiences to broader historical issues. In this account, however, he is constrained by the stylistic conventions of traditional family history, which require that one faithfully record every family name, detail, or event, but not probe too deeply into more controversial aspects of a family's past.

The quality of the research and the sophisticated knowledge of the larger historical context place this biography in a class above conventional family histories. One wishes, however, that the study had been shortened, given thematic rather than chronological focus, and used to explore the significance of broader issues, both of Woolley's own life and of the pioneer experience in the West.

LAWRENCE FOSTER

Georgia Institute of Technology

DANIEL HODAS. *The Business Career of Moses Taylor: Merchant, Finance Capitalist, and Industrialist*. New York: New York University Press. 1976. Pp. xvi, 356. \$17.50.

The Business Career of Moses Taylor adds to our knowledge of nineteenth-century American business history in two ways: we learn how a practicing member of New York City's merchants' guild from the 1830s to the 1880s conducted his operations, and his career illustrates the classic intragenerational transition from merchant to financier and industrialist. Like so many of his counterparts, Taylor had the uncanny ability to seize opportunity out of adversity (he invested heavily in downtown Manhattan real estate—in 1842), followed opportunities wherever they led him, was extremely careful and methodical (he kept his own set of account books at home), and filtered his business relations through financial controls. As a result, this capitalist parlayed his \$15,000 stake in 1832 into \$40,000,000 or so by his death fifty years later.

Both the strengths and weaknesses of the study lay in its close following of the Taylor papers in the New York Public Library. The reader gains insights into Taylor's self-conscious progress in the world, and is swept along by his apparent ability to be at the right time and place. But Hodas follows these sources too closely and exclusively; and the result is excellent descriptive information interspersed with lame analyses or immature assessments of significance. Too often, Hodas draws such conclusions as "His [Taylor's] policy always was to support wholeheartedly any project or firm in which he invested" (p. 147). Doesn't everybody? Essentially, the study is too thin; it wants fleshing out—at least with additional primary information about the business contexts in which Taylor made

his decisions, and preferably also with informed judgments about Taylor's decisions and career progress.

Yet the material presented here is of enormous interest. Perhaps the most important thing we learn is that depressions may actually have furthered capitalism more than new technology did. Taylor represented New York's merchant-bankers who accepted the Jacksonian doctrines of hard money and the quantity theory of money (best articulated by Freeman Hunt) because it suited their position as international traders and their city's financial dominance over other American markets. They understood why and how the deflations of 1837–43, 1857–60, and 1873–79 presented unique investment opportunities because they tracked the issuances of paper money against specie holding, commercial paper against the volume of business activity, the rates of bills of exchange against the volume of America's exports, and followed the fluctuations of price levels as well as the reasons for them. Taylor, at least, invariably increased his debt and equity interests, and purchased real estate, during such times.

Apart from such considerations, the book has an attractive format, several illustrations, notes and bibliography, and few typographical errors.

GARY BROWNE

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RONALD G. WALTERS. *The Antislavery Appeal: American Abolitionism after 1830*. Baltimore: Johns Hopkins University Press. 1977. Pp. xvii, 196. \$11.00.

Studies by writers like Donald, Kraditor, Lerner, Perry, and Wyatt-Brown have developed increasingly sophisticated analyses of antislavery and its place within antebellum reform. Now Ronald Walters attempts a major synthesis of abolitionism and its relationship to and place in that reform mainstream. The result is a very good book.

Linking conceptually and substantively the major themes of abolitionism with those of other movements such as revivalist religion, health reform, temperance, child and family nurture, perceptions of sexuality, and the concern for economic responsibility, Walters argues that abolitionists shared simple overarching tenets: immediatism, a willingness to devote time and energy to antislavery, and total commitment to blacks' civil equality with whites. He also hazards that antislavery can be understood only if these tenets and their relationship to concurrent general reform values are recognized; and, thirdly, that this active relationship provided an ample dynamic to sustain antislavery momentum. Finally, he contends that the

interrelationship illuminates both the antislavery crusade and other antebellum reforms.

The argument is ingenious and Walters applies it thoughtfully. Antislavery commitment was closely related to the intensity of both religious orthodoxy and evangelical enthusiasm. The ideal of moral elevation, central to antislavery thought, had parallels in other movements for human equality, on behalf of personal moral responsibility, and against a multitude of sins. The need to crush the slavocracy paralleled obligations to curb lusts for sexual, economic, and political power. Finally, abolitionists' condemnation of slavery's shattering any community among bondsmen paralleled the Middle Period's condemnation of that which disrupted the family unit. In these and other ways Walters demonstrates the "resonance between the most persistent themes in antislavery literature and conditions in antebellum America."

While succeeding, he also generates nearly as many problems as he solves. If, for example, abolitionism can be reduced to a few postulates common to antebellum reform generally, will not everything, Walters himself asks, "end up looking the same"? Stephen S. Foster, Samuel J. May, Amos A. Phelps, Maria Chapman, and James G. Birney espoused abolitionism in widely different ways. Walter's assertion that "we do not need to know what brings individuals to a movement in order to understand the movement itself" (p. xii) cannot stand unchallenged. Without the Tappans or Garrison what would New or Old Organization have been like? Similarly one cannot dismiss black abolitionists because they "had" to be abolitionists. Not only did colonizationists, nationalists, and rebels variously abound in black antislavery ranks; but even collectively those ranks were thin. And surely it is important that the Garrisonians attacked black and white apostates on widely different grounds.

In his attempt to distill an intellectual history of antislavery within the overarching general reform context, Walters has, in brief, so telescoped and stylized that he has bled both of their dynamic qualities and human dimensions. He has saddled antislavery with more hypotheses than it can reasonably sustain. These weaknesses aside, however, he has essayed a synthesis which few have attempted. In so doing he has raised important questions—and that indeed makes his a good book.

WILLIAM H. PEASE
University of Maine,
Orono

JAMES PICKETT JONES. *Yankee Blitzkrieg: Wilson's Raid through Alabama and Georgia*. Athens: University of Georgia Press. 1976. Pp. xiv, 256. \$12.00.

In March and April 1865, Major General James H. Wilson led some 13,000 Union cavalrymen across central Alabama and into Georgia. It was more than a raid. The aim and the result was the elimination of the central South as a "last ditch" for the Confederacy. The twenty-seven year old "boy general" had organized a magnificent striking force of veteran troopers, well mounted and armed with repeating carbines. Opposing them were remnants of Bedford Forrest's command and sundry home guard units. Wilson's tactics had earlier been perfected by Forrest. Neither harbored a romantic notion of cavalry; it was to be utilized as mounted infantry. The carbines gave it firepower; the horse, mobility. It had taken almost three years for the Union High Command to come to use cavalry in this manner. Wilson looked upon himself as an innovator in this respect, and perhaps he was.

James Pickett Jones handles the narrative details of the campaign in good fashion. He sets the context of events, with full appreciation of the command questions involved, and includes the reactions of individual officers and enlisted men. Wilson was particularly fortunate in that his division commanders were capable, even brilliant, and his men spirited, disciplined, and effective. The term "disciplined" must be used with qualification, because the troopers evinced little regard for the rights of civilians or, unless coerced, for the niceties of military courtesy. On the march and in battle, however, they performed admirably and in the end won the praise of their commander. Fewer qualifications must be attached to their effectiveness, even considering that their opposition was not the strongest. They scattered Forrest's command, destroyed military supplies, railroads, and Alabama's iron industry, and, to cap it all, captured Jefferson Davis.

Jones may attribute too much significance to this raid, both in the context of the Civil War and of its contribution to tactics in future wars, but this should be interpreted as a minor criticism of an excellent monograph.

JOHN T. HUBBELL
Kent State University

STEPHEN B. OATES. *With Malice Toward None: The Life of Abraham Lincoln*. New York: Harper and Row. 1977. Pp. xvii, 492. \$15.95.

A Lincoln revival of sorts seems to be under way. After a quarter century during which only two comprehensive one-volume Lincoln biographies appeared—Benjamin P. Thomas' (1952) and Reinhard H. Luthin's (1960)—three new lives have been scheduled for publication in the single year 1977.

The first of these in order of appearance is the work of Stephen B. Oates, author of a highly acclaimed John Brown biography as well as several other books. Oates announces his intention to present the "real Lincoln" in place of the "Lincoln of mythology" and to reveal, among other things, the man's "racial views in the context of his time and place." Oates says he has "utilized scores of published source materials and unpublished manuscript collections" in addition to numerous scholarly studies. The jacket blurb describes the book as "lyrical, engrossing and thoroughly moving."

Certainly Oates' style is racier than Thomas', and much more so than Luthin's. In spots it may be too racy for the tastes of some. It is marked if not marred by slangy expressions (Lincoln "wasn't too interested"; he "griped"), over-familiar terms (the rather hard-bitten Montgomery Blair is always "Monty"), mixed metaphors ("waded into Lincoln's speech . . . with both fists, flaying it"), misusages ("convince" for "persuade," "by-elections" for "mid-term elections"), and even malapropisms ("expostulated" for "expounded"). The author, a far more skillful writer than most professional historians, deserved a better copyeditor.

Documentation is provided in unnumbered "reference notes," thirty-eight pages of them, rather than in traditional footnotes or other specific citations. Very few manuscript sources are cited; essentially the book is a synthesis, and a remarkably comprehensive one, of recent scholarship. As such, it has an up-to-date quality that both Thomas and Luthin have lost. To illustrate, it includes in its index, as neither of those two does, entries for "Frederick Douglass," "Negroes," "race," and "racism." At some points, where scholarship remains divided or uncertain, the Oates account has an assurance and a precision that the sources hardly warrant.

In other respects the treatment is curiously imprecise. Republicans long familiar as Radicals or Jacobins—Charles Sumner, Benjamin F. Wade, Zachariah Chandler, and the rest—are rechristened "liberal" Republicans. Even William Pitt Fessenden, usually known as a Conservative or, at most, a Moderate, is characterized as a "popular liberal." Such terminology confuses more than it clarifies; it results in a kind of homogenized history. It is also anachronistic, for some of the Radicals, including Sumner, were of course to join the so-called Liberal Republican movement of 1872, but they did not refer to themselves as Liberal Republicans in Lincoln's time. Another anachronism is the frequent repetition of the term Founding Fathers as if it belonged to the language of Lincoln. There is no record of his ever having used the

phrase. Indeed, it derives from the period and style of Warren G. Harding.

The book focuses mainly on Lincoln and his immediate surroundings, though it does recount at some length events of interest to him, especially military events, in which he had no direct part. The tone is impartial, even aloof, yet readers are likely to get from the book a warm sense of sympathy with Lincoln in his many trials. They may be left wondering, however, why he is generally considered the greatest of American presidents. His greatness does not quite come through. Perhaps that is an aspect of the mythology that Oates has undertaken to dispel.

RICHARD N. CURRENT
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LEROY P. GRAF and RALPH W. HASKINS, editors. *The Papers of Andrew Johnson*. Volume 4, 1860–1861. Knoxville: University of Tennessee Press. 1976. Pp. xxxvii, 745. \$20.00.

Volume four (1860–61) of this excellent set covers, in effect, Andrew Johnson's final years as a legislator. Congress confirmed his appointment as military governor of Tennessee on March 4, 1862; and he served in the Senate in 1875 for only a few weeks before his death. In this volume Johnson stands alone in Congress as a Southern Unionist.

To the expressed regret of editors Leroy Graf and Ralph Haskins, this volume shares with its predecessors a shortage of Johnson letters. (There are only twenty-four available in this period.) Unlike the predecessors, volume four contains no personal Johnson letter in which the Senator unburdened his deepest feelings. Instead there is a flood of letters, mostly from strangers; and the editors are selective. Some of the letters are illiterate, some pedantic, some wise; but the most poignant are those from border states where loyalties were cruelly divided.

As in the preceding volumes the cross-referencing, indexing, and identifying is exhaustive. Let Johnson refer to his blood as "a willing libation upon the altar of my country's liberty" (p. 704), and the editors suggest Sir Thomas More as a possible source. The salutation in one case is "Deir Sirr" (p. 109), and the authors identify a Decaturville, Tennessee tailor who went on to write, "your proshin that you have taken is the Shalvation of ouer cuntry." A letter from Nashville tells of "much fear and anxiety among the people" (p. 383), and the editors identify a post office clerk as the author. A letter urges Johnson to come to Blountville, Tennessee to make some fence sitters "safe union men" (p. 419), and the editors discover

that the writer later served the Confederacy. His name was R. P. Fickle.

To the credit of the sponsoring organizations, the editors continue to include Johnson speeches readily available in other sources. This gathering of all Johnson material in one place will be of great value to students in the future. When the first volume of this set was reviewed in these pages (*AHR*, 73 [1968]: 1637) the reviewer wrote that it "meets the highest canons of scholarship." Volume four maintains this eminent standard.

JOHN N. DICKINSON
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HAROLD M. HYMAN. *Union and Confidence: The 1860s*. New York: Thomas Y. Crowell, 1976. Pp. 302.

Union and Confidence: The 1860s is the third volume in "a retrospective series" commissioned by Dun and Bradstreet Companies, Inc. Harold Hyman, among the nation's better-known Civil War historians, was requested to probe "the business aspect of the 1860s as well as the political and social history of the era." Hyman stated his objective in more scholarly fashion—"to examine the adequacy and appropriateness of certain public and private-sector institutions in relation to the hazards and opportunities of the 1860s." He adds that "the Union not only enjoyed but deserved the rebirth of confidence in its durability and utility; and, as confidence returned, both public and private institutions functioned more vigorously and effectively, further strengthening the Union."

Dun and Bradstreet imposed a high impossible task (combining economic, social, political, and military history of the war into a text of three hundred pages) upon a reputable historian, and the result is a hodgepodge of topics loosely held together. The theme of "confidence" is an elusive and ephemeral one, more contrived than real. Confidence, especially in the North, ebbed and flowed, dependent, in the main, upon Union victories. There is no mention of the Union Leagues or the Loyal Publication Society, both so important in maintaining Northern morale. Nor is there an analysis of how faith in Divine Providence and nationalistic ideals sustained confidence in the darker days of the Civil War or in the latter years of the 1860s.

The book, in a measure, is a compilation of generalizations, stated forthrightly and competently. The following illustrate the tone and tenor of the book: 1.) "Davis initiated the bombardment [of Fort Sumter] because he shared the white South's commitment to the Confederacy and its arrogant attitudes toward the North" (p. 74); 2.) "Fewer political restraints operated in Dixie because political democracy was feebler there" (p.

96); 3.) "The powers of states did not diminish as the nation's war functions increased" (p. 193); 4.) "Imbued with an overblown sense of the office he occupied, Johnson stretched presidential prerogatives to unprecedented lengths, thereby creating crisis" (p. 250); 5.) "Then, in February 1868, Johnson suddenly attempted to seize control of the only means the nation possessed to effect Reconstruction, the army" (p. 261).

All Civil War historians will not agree with every interpretative comment or generalization. Some will disagree with more than others—and that is the way it should be. But all will welcome *Union and Confidence* as a provocative and scholarly book.

The text seems to be written for the scholar rather than the general reader. Forty illustrations, judiciously chosen, enhance the book. Footnotes are rather rare, and the index needs more sub-topics. Dun and Bradstreet deserve commendation for underwriting this contribution to historical scholarship.

FRANK L. KLEMENT
Marquette University

RICHARD N. CURRENT. *The History of Wisconsin*. Volume 2, *The Civil War Era, 1848-1873*. Madison: State Historical Society of Wisconsin, 1977. Pp. xiv, 639. \$20.00.

Celebrating the American Revolution Bicentennial, the Wisconsin State Historical Society commissioned an ambitious six-volume work. This second volume probes Wisconsin history from statehood, May 29, 1848, to the eve of the depression of 1873. The Civil War is a theme, but not a central motif, in a score composed of the geography, society, economy, education, religion, politics, and optimism of Wisconsin. If that seems a bold undertaking even for a twenty-five year period in the history of one state in fifty, it is.

At times, when reading the volume, one might speculate if it does not more properly belong on the shelf with the encyclopedias, rather than the histories. Were it not for Richard Current's easy style, skill in fitting the elements together, and willingness to pause occasionally to tell a story, it would emerge as even more encyclopedic than it is. The first third of the book includes a tour to and within the Wisconsin of 1848, including the transportation routes, natural resources, genesis of commerce; the background, religion, and lifestyles of whites, blacks, and Indians; the formation of counties, towns, churches, schools, newspapers, libraries, and public services.

Much more at home with the period covered in the second third of the volume, Current examines Wisconsin politics, from Democratic dominance in 1848 to Republican control in 1856, relating the

impact of and response to national issues. The effect of the recession of 1857–61, Wisconsin's use of states' rights (as in the Booth cases), the election of 1860, reaction to secession, outbreak of Civil War, the raising of troops and their composition and campaigns, and the war on the home front all follow in a comfortable, but lively, manner. In the final third, Current picks up the threads of the first, with the emphasis on growth and change. The volume concludes with a survey of Reconstruction era politics in Wisconsin, and ends as the economic collapse of 1873 begins.

The author primarily used the rich resources of the Wisconsin Historical Society, combed unpublished masters theses and doctoral dissertations for relevant subjects, mined myriad local histories and a wealth of secondary literature. The bibliography is excellent, extensive, and invaluable for students of Wisconsin history. Current is at his best analyzing the political scene, with Democratic schisms and Whig and Free Soil coalitions, the birth of the Republican Party, its 1836 triumph in Wisconsin with Frémont, and its sometimes uncertain hold on the state for the remainder of the era. Black suffrage proposals failed in 1849, 1857, and 1863, but in 1866 the Wisconsin Supreme Court granted blacks the vote by reversing the 1849 election. The composition of its troops in the Civil War clearly reflected the tremendous impact of immigration on Wisconsin: forty percent were foreign born. Yet even so comprehensive a work as this cannot treat every subject, and so we learn nothing about the fate of "Badger Boys in Blue" who were taken prisoner.

The bicentennial of the United States occasioned the publication of a number of multivolume and single-volume state histories. The Wisconsin Historical Society set an admirably high standard for its contribution, and Current's volume provides an excellent scholarly example for subsequent publications both from Wisconsin and elsewhere. The task of achieving a sound national synthesis based on state history would be immeasurably eased were all state histories of this quality.

ROBERT H. JONES
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GLENN M. LINDEN. *Politics or Principle: Congressional Voting on the Civil War Amendments and Pro-Negro Measures, 1838–69*. Seattle: University of Washington Press, 1976. Pp. xvii, 88. \$12.50.

Glenn M. Linden, whose work on the Radicals is widely and favorably known, attempts in his most recent study to answer questions both qualitatively different and broader in scope than those he has dealt with before. His continued reliance on congressional voting analysis and a considerable over-

lap in time period are the only elements that connect *Politics or Principle* with his previous work. This book must be viewed, therefore, much more as a venture onto new ground than as a simple extension of his earlier articles.

Linden wishes to test the validity of the assertion that Republicans who voted for the Fourteenth and Fifteenth Amendments did so primarily for political reasons. He proposes to do this by determining the consistency with which individual congressmen supported the "pro-Negro" position on two groups of measures: 1.) those "pertaining to Negroes," 1838–64, and 2.) the various votes related to the Thirteenth, Fourteenth, and Fifteenth Amendments, 1864–69. Congressmen who cast roughly the same percentage of their votes on the "pro-Negro" side in both eras, Linden argues, cannot fairly be categorized as unprincipled. He concludes that "most congressmen were consistent in their voting on all measures relating to the Negro in the years 1838–69," and hence cannot validly be charged with unprincipled partisanship.

Unfortunately the study is severely flawed in both concept and execution. No consistency is established by the examination of these votes because there are few truly common elements in the prewar issues and the three Reconstruction era amendments. The Fifteenth Amendment, for example, embodied an issue far more precisely definable than the vague tag "pro-Negro" implies. If one would find whether congressmen acted consistently in voting for the Fifteenth Amendment, one must determine if they supported black suffrage in the antebellum era. We know the answer to that question—most did not. Congressmen strongly supporting the Fifteenth Amendment might be expected to favor the "pro-Negro" position on a variety of other issues. And, of course, they did so; but so did many who were much less enthusiastic about the Fifteenth Amendment.

All of which leads inevitably to an insuperable objection to Linden's research design. Consistency may well be a jewel or, alternatively, "the hobgoblin of little minds," but it is no indication of principle. Many individuals are consistently unprincipled and others, adhering rigorously to principles clear to them, would appear to an investigator examining their actions in the light of another principle to have pursued a highly inconsistent course.

Furthermore, the two primary analytical components are so casually constructed as to make Linden's findings meaningless. The inclusion of the votes on the Thirteenth Amendment (which are largely irrelevant to the charge of unprincipled partisanship) grossly skews the percentages in this area, especially in the House, where the seven Thirteenth Amendment votes constitute almost

thirty percent of all those listed on the three amendments. The records of two Ohio representatives will serve to illustrate the potential for distortion. James A. Garfield's record shows him supporting the "pro-Negro" position on the amendments three-quarters of the time, but the figure is less than two-thirds if his Thirteenth Amendment votes are excluded. Under the same circumstances Rufus P. Spalding's level of support for the "pro-Negro" position declines from three-fifths to just over two-fifths. In the antebellum era the problems are even greater, both because of the longer time span and the diversity of issues included. It is patently impossible to accept as valid the results derived from an analysis that treats as equally "pro-Negro" such wildly different positions as espousal of Negro voting in Oregon and support for the American Colonization Society's efforts to transport free blacks to Liberia!

Politics or Principle is, moreover, riddled with factual errors which destroy confidence in the accuracy of the statements made. William G. Brown was a representative from West Virginia, not Wisconsin; John S. Carlile served as a senator from Virginia and West Virginia, but never from Kansas; Henry Grider was elected by Kentuckians, not New Yorkers; Georgia representative Samuel F. Gove was a Republican, not a Democrat; and William Wright served as a Unionist, not Democratic, senator from New Jersey, not Kansas or Iowa. These (and others not listed) are not mere unfortunate typographical blunders or the result of the uncritical acceptance of incorrect party designations given in the *Biographical Directory of the American Congress* (though there are some of these as well). They are consistent errors that affect Linden's sectional and party vote analyses. The correction of Gove's party affiliation, for instance, changes the level of southern Democratic support of the "pro-Negro" position on the Fifteenth Amendment votes from 46% to 7%.

There are other errors even less defensible. Senator James H. Lane of Kansas could hardly have cast the votes in the Thirty-fifth and Thirty-sixth Congresses credited to him, when he was not seated in the Senate until the spring of 1861. The dramatic shift of New York's Congressman Ward from a 0% "pro-Negro" position in 1838-64 to a 76% level in 1864-69 is less surprising when one realizes that the earlier votes were cast by Democrat Elijah Ward and the later ones by Republican Hamilton Ward.

The errors of greatest magnitude, however, are associated with vote tabulation. I have checked three of the four major raw data tables on which this study rests. With distressing frequency the aggregations printed do not conform to the votes recorded. In the table showing the Senate votes on

the amendments, for example, the votes of sixty-seven Republicans are recorded. In sixty-three cases (94%) the aggregations do *not* conform to the votes listed. The frequency of error in other components is lower—on the comparable table for House votes the error rate among Republicans is 55%—but rarely insignificant. I have not checked the original votes but internal evidence strongly suggests that votes on several issues are improperly recorded. Thus, in a number of cases the aggregations may be correct and the vote entries erroneous. A simple correction of the flawed entries, however, would not resolve the problem, for a number of aggregations that now appear to be accurate would then become inaccurate.

It is a fact greatly to be regretted and personally painful to record, but this book is wholly unacceptable in its present form. At the very least the demonstrable errors should be corrected and the analyses reaccomplished in accordance with such changes. The study would, in my opinion, still be largely irrelevant to the issue that it is designed to address, but the data on which it is based would at least be accurately reported.

LEONARD P. CURRY
University of Louisville

W. SHERMAN SAVAGE. *Blacks in the West*. (Contributions in Afro-American and African Studies, number 23.) Westport, Conn.: Greenwood Press. 1976. Pp. xvi, 230. \$14.95.

In this volume, W. Sherman Savage climaxes fifty years of scholarship on the role of blacks in the trans-Mississippi West. He has focused on the periods from 1840, when they first appear in Western census returns, to 1890. The work is topically organized, with chapters on migration, slavery, military service, occupations, civil rights, politics, education, and social life. Each chapter is rich in primary sources, particularly black and Western newspapers, territorial and state archives, and individual manuscripts.

Some recent works on blacks in the West have emphasized themes of black history—racism, discrimination, denial of civil rights—and used these to refute a Turnerian interpretation. Savage's work is of the traditional Western genre. His longest chapter is on such "romantic" occupations as fur trading. He stresses the accomplishments of individual blacks and spends less time recounting group discrimination or white prejudice. Thus, the black experience is seen as supporting Turner's views. While acknowledging that for blacks the West was not a "Promised Land," Savage concludes that it offered them fewer restrictions and greater opportunity than did other regions.

The objective tone and the frontier framework

are also weaknesses of this work. By omitting most examples of prejudicial attitudes toward blacks Savage fails to convey a complete sense of their disadvantages, or of the moral indignation that motivated the Colored Conventions. By ending his history at 1890, he fails to recognize several trends which were already altering the receptiveness of the West to blacks. Depression was limiting their economic opportunities and population growth and contributing to a growing hostility to non-whites, as Elmer Rusco has observed. Insofar as the West held promise for blacks, it would be less in rural areas than in cities such as Los Angeles, yet the land boom of the 1880s is not mentioned.

Many of these oversights must be attributed to the author's use of secondary literature. His bibliography is rich in books and periodicals through the 1940s, but it contains little of the literature on blacks in the West that has come out since 1960. His chapter on slavery offers no analysis of the relationship between antislavery and antiblack attitudes, as noted by Berwanger. In treating civil rights the interplay between different rights issues and the Colored Conventions, as treated by Lapp and Fisher, is not evident. Nor is reference made to the parallels between white attitudes toward blacks and toward other minorities, noted by Luther Spoehr and Daniels and Kitano.

In sum this work will not offer scholars a new analytical framework for Western black history, nor does it evaluate the most recent interpretive writing in this field. But it ably summarizes the accomplishments of blacks in this region and provides a rich body of biography and sources upon which future scholars might build.

LAWRENCE B. DE GRAAF
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Fullerton

NELL IRVIN PAINTER. *Exodusters: Black Migration to Kansas after Reconstruction*. New York: Alfred A. Knopf, 1977. Pp. viii, 288. \$12.95.

The first significant migration northward of the South's former slaves took place in 1879-81, shortly after the end of Reconstruction. Thousands of "Exodusters," as they were called (because of the parallel between their plight and that of the Hebrews who fled ancient Egypt), departed Louisiana, Mississippi, Texas, and Tennessee; their destination was Kansas. This "African hegira," as one observer termed it, was the distant forerunner of the Great Migration of black people to the North which occurred during the First World War. A genuine folk movement, the Exoduster migration has hitherto been undeservedly ignored. Now, Nell Irvin Painter has produced a book which rescues the Exodusters from obscurity and

demonstrates her considerable talents as a researcher and writer.

Painter, in the early chapters, gives a vivid, informative outline of Southern race relations at the end of Reconstruction. Most of her material has been mined by other historians, but she offers some new and compelling insights. Particularly useful is her chapter on "Fictions of Black Life and Southern Race Relations," in which she convincingly demolishes some of the stereotypical assumptions about that period. And in a subsequent chapter on "Politics and the Color Line," she correctly points out that blacks "did not automatically vote Republican out of gratitude for emancipation," but rather for the more practical reason that during and after Reconstruction the Republican, more than the planter-dominated Democratic Party, "represented the interests of working people" in the South.

The oppressive tenant farming system which gripped most blacks of the lower Mississippi valley was reason enough to look for a new homeland elsewhere. But when to economic desperation was added, following Reconstruction, an increased level of violence and political coercion, the burden became too much to bear for many thousands of blacks. Some talked of migration to Liberia—to the African homeland. But Kansas was, as Painter writes, "a more feasible alternative." Homestead land was still available there, the soil was rich, and Kansas by its history "was the quintessential Free State, the land of John Brown." For most of the 20,000 or more blacks who went there from the Deep South in 1879-81, Kansas turned out to be no Canaan, but it was at least a place where there was some chance to realize their human potential.

WILLIAM I. HAIR
Georgia College

DAVID A. GERBER. *Black Ohio and the Color Line, 1860-1915*. (Blacks in the New World.) Urbana: University of Illinois Press, 1977. Pp. xii, 300. \$14.95.

Even though the late William B. Hesseltine used to growl that all history is local history, few colleagues believed him, and, until recently, few scholars wrote and interpreted local history in a larger context. Now as urbanists and ethnicists, among others, have begun to uncover exciting new data, they are documenting the centrality of local units in national history.

David Gerber has done this with intensity and skill. He has mined manuscript and archival collections to add significant elements of private comment and public relatedness to this narrative. He argues persuasively that Ohio blacks after the Civil War were integrationists who worked within es-

established frameworks to widen opportunities for the race. In politics, this meant the Republican party, where narrow margins in state and local elections often gave black electors a balance of power which they used to achieve party recognition as well as appointive and elective office.

In economic areas, blacks were restricted, but the postbellum generation played up the success of "representative" black persons, rejected separatism, and viewed the future with optimism. Ohio blacks in the period before 1900 fought successfully for integrated schools, eschewed massive concentrations in particular city wards, supported the passage of a weak state civil rights act, and, generally and genuinely, believed that they were moving toward equality with whites.

Gerber asserts that the reaction which set in sometime after the 1880s was intimately related to the rise of legal and violent racism in the South. It circumscribed Ohio blacks' influence politically, leveled out any previous improvement in economic activity, encouraged separatism in some school systems and churches, accelerated the growth of large black communities in cities, discouraged blacks from using hotels, restaurants, and transportation on an equal basis with whites, and closed off to blacks the facilities and services of social agencies like the YMCA.

This thesis, which is not new, deserves close attention because it has national implications. Other states and regions north of the Mason-Dixon line may show comparable patterns. The political ties of Ohio blacks were influential in Columbus and, eventually and briefly, in Washington. Ohio was an apparent outer fringe of the Booker T. Washington network and W. E. B. DuBois' organizing efforts. The black press, notably the *Cleveland Gazette* and the indomitable Harry Smith, had impact outside of Ohio.

Yet the thesis—that the black world in Ohio turned from open, integration-oriented optimism to closed separation-oriented pessimism sometime in the 1890s—needs to be challenged. Gerber bases his interpretation on changing white attitudes about blacks: "Slowly in the 1890's, and more rapidly thereafter, Ohio's race relations deteriorated." The evidence is strong but not convincing. What happened was that the black population slowly grew and became more visible (even before 1915). A new generation of young leaders appeared—largely professional men and women striking out in areas where blacks had not previously settled—and there was a growing sense of race nationalism. The so-called integrationist optimism before 1900 was mostly veneer, voices crying out in the wilderness. The earlier complaints were not too much different from those of the new generation whose reactions were tempered by their

greater education, their sense of the need for black cultural appreciation, and their youthful inexperience.

Gerber's own comprehensiveness supplies additional evidence that the pre-World War I era was not all that gloomy. Separate institutions emerged, and the church prospered and even gave birth to some social agencies. Attorneys and physicians set up practice, and black businessmen, battling the odds against survival, fought for the black dollar. The old-time politics began to dissolve before the thrust of Progressivism and mobility, but both blacks and whites had adjustment difficulties during the transition. "Once again," Gerber concludes about this period, "the migration and urbanization trends had shown their potential for exerting a dynamic influence on racial life."

Whether or not the Gerber thesis will stand without qualification, it is a challenging concept set forth in a judicious and scholarly way, a study which confirms the truism that all history is local history.

LESLIE H. FISHEL, JR.
Heidelberg College

JEAN WILSON SIDAR. *George Hammell Cook: A Life in Agriculture and Geology, 1818-1889*. New Brunswick: Rutgers University Press. 1976. Pp. xvi, 282. \$11.95.

This book is a chronicle of George Hammell Cook's life and activities. As a chronicle it successfully introduces the reader to a man who was deeply involved in a variety of activities, including the Second New Jersey Geological Survey, the development of agricultural science in New Jersey, and the development of a scientific school at Rutgers. The author used sources available to her at Rutgers, but she made little use of materials available in other archival sources. While this resulted in an adequate chronicle, it negated deeper insight into Cook's career. Therefore, as an appraisal of Cook's influence and place in the development of science in the last half of the nineteenth century, the book is less successful. The historian is likely to be disturbed by the many possibilities that are suggested but not pursued by the author. Thus while we learn that Cook's interests in geology and agriculture were related, we do not find that this relationship is explored in a way that adequately relates Cook's activities to trends or ideas in the geological and/or agricultural sciences or that shows Cook's place in the overall state of these sciences. While we discover that Cook's examination of the sugar beet industry in Germany had some influence on New Jersey's attempt at sugar beet production, we do not learn how. Too many examples of this type can be cited.

Biographical details concerning people with

whom Cook worked at various times and in various projects are often included. This is helpful in many instances, but these details sometimes obscure Cook's story and appear to be irrelevant. The book contains extensive information about the history of Rutgers. Since Cook's career was intimately involved with this history, it is appropriate that the author has included much of the information. However, this too is sometimes overdone, detracting from the main theme.

The book has obvious faults which result from the narrowly defined character of the biography. The historian of American science will, nevertheless, find it useful in bringing together the diverse parts of Cook's career and activities.

PATSY A. GERSTNER
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SHERBURNE F. COOK. *The Population of the California Indians, 1769-1970*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xvii, 222. \$12.75.

This posthumous volume by Sherburne F. Cook offers scholars of California history and of North American aboriginal demography a unique, exhaustive compilation of data on California Indians. The book eloquently presents the data for temporal shifts in California Indian population size and composition, bringing together for the first time immense and varied data formerly available in scattered sources but seldom consulted. The result is an exhaustive, authoritative work that should become a basic reference for anyone interested in California aboriginal population.

The book begins with a review of past estimates of pre-contact population size, a lengthy discussion of the relevant data, and a new state estimate of 304,440. This figure is the most controversial element in the volume, since it represents an increase of 17 percent above Merriam's 1905 estimate (used by Mooney), 144 percent above Kroeber's 1925 figure, and 128 percent above Cook's own estimate of 133,500 in 1943. Although this high estimate will certainly be viewed with caution, Cook cross-checked his data whenever possible and has presented his logic and data in detail for scholarly examination, a commendable practice not always employed in estimating aboriginal numbers. Cook's figures obviously reflect exhaustive years of scholarship and generally should be trusted. However, he accepts Dobyns' speculation of a 95 percent hemispheric population reduction as "the rule rather than the exception." The extent to which this acceptance has biased interpretation of his own data should be given future scholarly attention.

The remainder of the book pulls together a wealth of rarely cited data on temporal change in population size, age distribution, racial composition (admixture), vital statistics, and population distribution. The result is a detailed, clear picture of the sweep of California aboriginal population history. The data document a severe population decline immediately after contact with an increase in an already high crude mortality rate. The nadir was reached about the year 1900 with the trend clearly reversing itself shortly thereafter. The rate of population increase has progressively accelerated from that time to present. This trend is well illustrated in Cook's table 15, where the birth-death ratio for California Indians increases from 0.085 in 1906 to 5.819 in 1970. If the trend persists, as Cook predicts it will, California Indians will go a long way toward regaining their once considerable numbers.

DOUGLAS H. UBELAKER
Smithsonian Institution

ANGIE DEBO. *Geronimo: The Man, His Time, His Place*. (Civilization of the American Indian Series, number 142.) Norman: University of Oklahoma Press. 1976. Pp. xx, 480. \$14.95.

Perhaps more than any other individual, Geronimo symbolized the terror and tragedy of the American West in the late nineteenth century. His final surrender in September 1886 marked as much as any event the close of an era in our history. For decades Geronimo and his people had resisted the incursion of Mexicans and Anglos upon their homeland. Geronimo lived for almost a quarter of a century following his capitulation. His last years served as far more than a footnote to his days as a warrior. They revealed once again a common determination of Native Americans to endure and eventually prosper.

Geronimo died a prisoner at Fort Sill, Oklahoma, when Angie Debo was 19. Nearly 70 years later she has written an exploration of Geronimo's "individual experiences, his motivations, his personal life and character." Building carefully upon the work of Dan L. Thrapp, and utilizing over sixty photographs and a wide variety of sources, Debo has presented as complete a portrait as we shall have of this complex, often misunderstood man.

Geronimo's actions brought sorrow and death to many in the Southwest. Yet as Debo reveals, his years also were characterized by disaster: the deaths of his wives and children, the loss of his compatriots, the forfeiture of his native land. Debo's account helps us to understand the kind of ferocity which distinguished Geronimo's efforts and the deepening desperation which gripped his people's lives.

In addition to untangling the complicated chronicle of Geronimo's life prior to 1886, Debo has devoted fully a third of her biography to his imprisonment in Florida, Alabama, and Oklahoma. Only then could some people begin to see him as a man who showed kindness to his friends, who possessed an inquiring mind and who proved to be a hardheaded entrepreneur. He was exploited, to be sure, but he also used those situations whenever possible to his advantage. In another culture, Debo remarks, he would have emerged as a captain of industry.

Debo readily admits that it may not be possible to write a definitive biography of Geronimo. Too much time has elapsed; too many Native American sources now are silent. But we are unlikely to obtain a more fair and thorough study of this man, his time and place.

PETER IVERSON
University of Wyoming

JOHN L. SHOVER. *First Majority—Last Minority: The Transforming of Rural Life in America*. (Minorities in American History.) DeKalb: Northern Illinois University Press. 1976. Pp. xix, 338. Cloth \$12.50, paper \$5.00.

This work, the last book by the late John L. Shover, is not, strictly speaking, about farmers as a minority group. It is, rather, an examination of the technological and commercial changes in agriculture which transformed the following: farming into agribusiness, Mom and Dad stores into supermarkets, and simple marketing procedures into vertically and horizontally integrated processing conglomerates. A by-product of this dynamic change was, of course, the dramatic decrease in the actual numbers of farmers in the United States.

The stated purpose of the book is to "capture and synthesize the scope, the importance, and the effect of . . . one of the most significant and extensive transformations of our day." To accomplish this the author first focuses on rural life and farming conditions prior to 1945 via a series of case studies: Scioto Township in Ohio, Bedford County in Pennsylvania, Tarpleywick farm in Iowa, and Stadtfeld's farm in Michigan. These studies, although not sufficient in number and variety to justify vast generalizations, nevertheless do indicate a steady progression in farm technology. Also present is a strong feeling of nostalgia for the simpler days of a bygone era.

The second part of the book is devoted to an examination of rapid technological innovation following World War II; the transition of small, family-owned farms into large agribusiness; the repercussions of federal management of agriculture

since the New Deal; and the causes (and possible cures) of the present world food crisis. This latter portion of the study, in particular, mingles subjective opinions, personal value judgments, polemics, and sociological insights with historical data, reasoned analyses, and logical deductions.

The author obviously dislikes bigness, which is a fact of life in modern agriculture. He is very critical of the so-called "oligopolistic food processing corporations," but neglects to mention that such large-scale operations make food in the U.S. the best and cheapest in the world. Likewise, Shover claims Agriculture Secretary Earl Butz manipulated government farm policy for the political advantage of the Republicans, but he never points out that the Democrats were even more skilled in winning rural votes with politically oriented farm programs. The chapter on the world food crisis, while a vital subject and interesting in content, really has little to do with the overall purview of a study aimed at explaining how the American farmer went from majority to minority status.

Was the "Great Disjuncture" (p. xiv), that is, the technological transformation in the post-World War II era, a greater historical watershed than the passing of the frontier in 1890? Shover thinks so. One might counter by asking, "Was not the process already in motion by the 1930s?" Hybrids were in use by then and farmers were steadily being "tractored off the farm" long before World War II. While the date of the "watershed" might be debated, this book is a valuable contribution to agricultural history. It is also a final tribute to one who must have loved farm life as it once was.

EDWARD L. SCHAPSMEIER
Illinois State University

MICHAEL SCHWARTZ. *Radical Protest and Social Structure: The Southern Farmers' Alliance and Cotton Tenancy, 1880-1890*. (Studies in Social Discontinuity.) New York: Academic Press. 1976. Pp. xi, 302. \$17.50.

This is the work of a sociologist committed to an interdisciplinary orientation that breaks down the boundaries between sociology, history, and economics. Michael Schwartz's book (along with Robert McMath's recent *Populist Vanguard*) examines the long-neglected Southern Farmers' Alliance, the largest and most radical southern proto-Populist movement. It was distinguished by its origins as a rank-and-file movement, and by its focus on economic protest, particularly boycotts and cooperatives, rather than on electoral campaigning.

Openly sympathetic to the tenants and small farmers who made up the rank and file of the

Alliance, Schwartz regards the book in part as an attempt to retrieve their insights and experiences. His principal theoretical antagonist is the social-psychological interpretation of protest movements as irrational, pathological expressions of psychic needs and wishes. Against this view, represented by Neil Smelser, Schwartz argues convincingly that the Alliance was a rational response to oppressive social conditions; the obstacles to rationality were found in a socioeconomic structure that limited knowledge and reduced communication, rather than in deep psychic structures.

The first section of the book analyzes the system of cotton tenancy that the Alliance sought to transform; the second is an organizational history of the Alliances which examines the social origins of members and the conflicts between leaders and the rank and file. The third section elaborates Schwartz's theoretical model, which centers on a "dialectic between movement and structure," and the fourth applies the paradigm in a concrete study of Alliance cooperatives and boycotts, concluding with a detailed study of a single North Carolina county. Schwartz's analysis of cotton tenancy is an explicit critique of Stephen Decanio's recent *Agriculture in the Postbellum South*. With Roger Ransom and Richard Sutch's forthcoming *One Kind of Freedom*, Schwartz argues that the conservative cliometricians' assumption that a free labor market existed in the South during this period is invalidated by the existence of peonage. His own use of the Gini Index to measure changes in inequality shows that mathematical tools need not lead to conservative conclusions. And Schwartz gives the first full analysis of the Alliance's 1888 jute boycott, one of the largest and most successful consumer actions in American history.

The book is conceptually much better developed and theoretically more ambitious than the work of most historians. But it does not have the same level of craftsmanship. The writing is uneven; awkward phrases such as "heavily cottonized areas" appear too frequently, as do sub-heads and sub-sub-heads, which break up the flow and detract from the significance of Schwartz's important argument.

JONATHAN M. WIENER
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Irvine

WALTER T. K. NUGENT. *From Centennial to World War: American Society, 1876-1917*. Indianapolis: Bobbs-Merrill Company. 1976. Pp. xv, 249. \$5.50.

This extended essay on modernization in the United States puts ideology in its proper context as a part of the nexus of social change, something students of Progressivism need badly to under-

stand. The ideology with which Walter T. K. Nugent is concerned is that of the native-stock white middle class, the element in American society without whose sanction no reform movement could hope to achieve even modest goals. Nicely balancing descriptive history with sharp analysis, the author shows how the dynamic, yet totally uncoordinated, changes of the post-Civil War decades produced the cultural crisis of the 1890s, which in turn persuaded the intellectuals of the time that new theories of the social order had to be developed and translated into political and legal realities.

These theories had as their pre-eminent purpose the triumph of moderate, well-considered reforms over the radical proposals being advanced at the turn of the century by socialists and other advocates of fundamental change in American society. People and institutions who had resisted change in the nineteenth century gradually perceived the existence of severe social problems and dangerous social tensions, then accepted the notion that rational solutions were both possible and desirable. For some, the road from classical liberalism and political mugwumpery to Progressivism was a short and relatively logical one; for others, it was a painful journey that entailed the abandoning of notions which once had seemed beyond the touch of man or history. Progressivism, the new liberalism of the twentieth century, was anchored in traditional middle-class morality and remained firmly committed to the dominance of bourgeois capitalism. But it did succeed in controlling social tension and in frustrating the forces of radical change. It also failed to solve, except in a superficial, piecemeal fashion, the internal problems of concentrated power, racial and ethnic dislocation, and crime and poverty, which continued to beset the urban-industrial society.

This brief book is full of insights and suggestions for further study that will benefit both mature scholars and classroom students. Like Robert Wiebe's *Search for Order*, moreover, it provides a fresh conceptual framework within which to view modern United States history. Because of a production flaw, however, it may not get the classroom use it deserves: the book literally fell apart in the hands of the reviewer as it was being read.

JOHN G. SPROAT
University of South Carolina

MORTON KELLER. *Affairs of State: Public Life in Late Nineteenth Century America*. Cambridge, Mass.: Belknap Press/Harvard University Press. 1977. Pp. ix, 631. \$17.50.

Public life in the United States from 1865 to 1900, the theme of this useful volume by Morton Keller,

is necessarily an ambitious topic. It requires attention to a host of political, economic, and social issues traced over a period of three and a half decades and studied at all levels of government—federal, state, and local. The structure of political parties, government regulation of corporations, campaigns to control individual social behavior, efforts to redefine national citizenship, the development of school systems and bar associations, and the evolution of executive, legislative, and judicial institutions—these are but a few of the subjects touched upon in *Affairs of State*. Keller attempts, therefore, to provide a comprehensive record of the issues dealt with by one or more of the American political system's constituent parts during the late nineteenth century.

Keller divides his study into two chronological periods, the first of which runs from 1865 to 1880. In the years immediately after Appomattox, he contends, the momentum of wartime centralization and governmental activism continued. Many Americans sought to affirm expanded conceptions of social equality and to establish a more nationalized economic order. The Fourteenth and Fifteenth Amendments, for instance, were representative of efforts to expand national citizenship. Railroad land grants and state and local educational reforms were but a few of the many instances of governmental activism. However, these newer tendencies in American life were soon retarded or reversed by countervailing forces of traditionalism. Consistent with recent scholarship on the Reconstruction era, Keller argues that the triumph of traditionalism was not simply an episode in regional history (that is, the "Redemption" of the South from Republican rule), but a phenomenon evident everywhere in the United States. Nationalism was checkmated by localism. Egalitarian social innovations withered under racist counterattacks. An ideological emphasis in party politics was supplanted by a focus on organizational imperatives. Widespread public adherence to laissez-faire ideas, the economic depression of the mid-1870s, and fear of social change all contributed to the strength of the traditionalist backlash.

Industrialism and its attendant economic problems brought new issues to the fore in the 1880s and 1890s, the second period covered by Keller. Labor legislation, government regulation of corporations, and social control of drinking, family life, education, poverty, and crime—all these subjects received markedly greater attention in public affairs in the last two decades of the nineteenth century. American society was deeply divided by these new issues, however. Traditional attachments to individualism, localism, and laissez-faire thought were powerful obstacles to the movement

toward greater social control and nationalism that accompanied industrialization. Thus, continued ambivalence toward the new, rather than the triumph of an industrial order, is the central theme in Keller's portrait of American public life from 1865 to 1900.

Affairs of State is a thoughtful synthesis of existing scholarship on post-Civil War American politics. Sections on individual topics are relatively brief, and in general the material presented on any given topic will be familiar to specialists in that field. The subjects covered are so numerous, however, that the book promises to become a standard reference work for anyone wishing reliable information on and an up-to-date analysis of American public life in the late nineteenth century.

GERALD W. MCFARLAND
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RUDOLF GLANZ. *The Jewish Woman in America: Two Female Immigrant Generations, 1820-1929*. Volume 2, *The German Jewish Woman*. New York: Ktav Publishing House and National Council of Jewish Women. 1977. Pp. xiii, 213. \$15.00.

While women comprised a larger proportion of Jewish immigrants to America than of most other immigrant groups, their particular historical experience has long been a mere footnote in the Horatio Alger saga of America Jewry. And of all American Jewish women, those of the nineteenth-century German migration to the United States—less numerous and less colorful than the Russian Jewish women who followed—have attracted the least attention. Rudolf Glanz's desire to restore women to American Jewish history, thus, is surely commendable.

Less commendable, however, is the result of his efforts. *The Jewish Woman in America* is difficult to read. It is really a collection of vignettes of the experiences and lifestyle of German Jewish women of the upper class, rather than a book. It has no apparent thesis and no discernible pattern of organization. Representative of its style is the concluding paragraph of the book: "On Jewish instruction we learn only little from the biographical sketches. In some cases, the Talmud Yelodim in Cincinnati is mentioned."

The author draws his examples of family life, society affairs, and education from sources scattered widely in nineteenth- and early-twentieth-century periodicals with little attention to chronological development. Dates of quotations are not given in the text, and citations from 1855 jostle others from the 1880s and 1890s in happy disregard of changed circumstances. Nor is Glanz prone to offer sustained analysis. Moreover, there is no evi-

dence in this book of the advances made by the scholarship of the past decade in the study of American women's history.

What Glanz does make available to the reader who is willing to ignore deficiencies of style and method is a wealth of descriptive material, culled mainly from contemporary periodicals, about Jewish social life in nineteenth-century America. In particular, the world of Purim balls, "at homes," and fashion is illumined in lavish detail. Glanz also describes the extension of women's activities in the public sphere, from their early involvement in philanthropic associations and Sunday schools to their participation in nonsectarian clubs and their founding of their own social service organizations. While Jewish women took little active part in the nineteenth-century women's movements, Glanz provides evidence that the American women's movements paid much attention to changes affecting the status of women in the Jewish community. By compiling a variety of sources, Glanz offers historians the raw material with which to begin an evaluation of the role and status of Jewish women in nineteenth-century America.

PAULA E. HYMAN
Columbia University

LEO HERSHKOWITZ. *Tweed's New York: Another Look*. Garden City, N.Y.: Anchor Press/Doubleday. 1977. Pp. xx, 409. \$12.50.

Leo Hershkowitz dismisses the conventional image of William Magear Tweed as a myth based on lies and unproven allegations against an innocent but vulnerable public servant. Using new sources, the author argues Tweed was the victim of cynical opportunists, sham reformers, irresponsible journalists, particularly those of the New York *Times*, dishonest contractors who bilked the city, Republicans intent on de-emphasizing their own scandals, and anti-urban groups, augmented by upper-class bigots, determined to denigrate Tammany. These men mocked justice through rigged juries, hand-picked judges, and tainted prosecutors.

Tweed's vindication came in a series of trials that merely convicted him for questionable dereliction of duty in failing to audit municipal bills, not personal theft or conspiracy. Then, too, Tweed formed no Ring, and no kickbacks or even a political machine existed; his income was legitimate, chiefly legal fees. Moreover, Tweed was a municipal reformer who freed the city from legislative meddling through a home rule charter passed without bribery, labored for educational advances, notably for Catholics, improved urban services, and set the pattern for the incorporation of Greater New York. Even his purported confession was sus-

pect because it came from a broken man who sought to barter penitence for freedom.

The author's ideas are intriguing but raise questions. If a political machine and Ring never existed, how should one treat the mass of historical evidence to the contrary? Hershkowitz admits that thievery was rampant, but adds that the final judgment about Tweed's involvement with misappropriated funds will remain uncertain because it is impossible to separate graft from profits. Yet much admittedly circumstantial evidence, linking Tweed to kickbacks, begs for another interpretation; as recent court cases underscore, the difficulty in proving a conspiracy does not necessarily prove the absence of one. Equally equivocal, if Tweed were innocent, how did the crusade against him garner such vast, bipartisan support? Why did Democratic ward leaders, with so much at stake, quickly abandon the guiltless Tweed? As for irresponsible journalism, the crusade against him began in the Democratic New York *World* over a year prior to the *Times*' revelations, and prestigious papers, including the *Herald* and *Evening Post*, claimed Tweed's legislative techniques involved systematic bribery. Finally, few recent historians deny Tweed's reformism, consider him all bad, or suggest his jail sentence was fair. Nonetheless, the questions they address—where did Tweed get his money and how did he plan to govern the metropolis—remain unanswered.

On balance, the book may not refute the conventional view of Tweed the corruptionist, but it does exonerate Tweed the man. As Hershkowitz illustrates, Tweed's lengthy and difficult trials, marked by his prosecutors' extraordinary vehemence, reveal a person who bore his travails with grace. In that sense, Hershkowitz has vindicated Tweed.

JEROME MUSHKAT
University of Akron

ANTHONY JACKSON. *A Place Called Home: A History of Low-Cost Housing in Manhattan*. Cambridge, Mass.: MIT Press. 1976. Pp. vii, 359. \$17.50.

Anthony Jackson's study of tenement housing in Manhattan focuses primarily on economic and legislative developments. Much of the ground covered from the 1850s to World War I is familiar: the New York State Tenement House Commissions of 1884, 1894, and 1900; the enactment and nonenforcement of tenement house acts beginning in 1867; the roles of Jacob Riis, Lawrence Veiller, and lesser lights; the model tenement house movement (philanthropy and five percent) launched by the New York Association for Improving the Condition of the Poor with its Workingmen's Home of 1855. The author's account continues into the 1920s, when a new generation of housing experts

lost confidence in traditional regulatory solutions to the housing problem, but could not yet generate alternatives based upon governmental subsidies (except for the feeble New York State Housing Law of 1926). The final section covers the public housing program launched in the 1930s and other forms of government housing assistance after World War II.

Jackson's labored and dry treatment of housing is less an exercise in historical analysis than a critique of the private housing market. As one plods through the bloodless prose, statistics, and citations from official documents, one eventually recognizes that Jackson is preparing an indictment rather than a history. His purpose is to demonstrate that the private market has never functioned efficiently. He dwells extensively on the filter-down concept, arguing that it tends to social segregation and deterioration of the housing stock rather than the orderly succession of the less affluent into housing vacated by higher-income groups. The problem, according to Jackson, is that the quantity of new housing is not sufficient for the purpose and does not lead to a significant reduction in the cost of older housing. The outcome is overcrowding, reduced maintenance, and decay. If anything, the filter-down process has encouraged middle-class suburban sprawl.

The failure of government housing assistance or subsidy programs, Jackson contends, is inevitable in the context of a housing market controlled by entrepreneurial norms. If government cannot control the supply of housing, it becomes merely a "residual supplier" or "charitable support." At every point, government is "undercut by the system within which it must operate" (pp. 303, 305, 307). It is "obvious" to the author that if private enterprise has no "moral duty" to supply adequate housing, it must be done through collective means.

There is, unfortunately, no necessary relationship between the validity of a critique and the validity of proposed alternatives. It is not even clear, programmatically, what the author's alternatives are. Condemnation of the private market combined with moral posturing rather leaves one stranded. If private enterprise has failed to provide all income groups at all times with housing appropriate to income level and life style, it does not follow that the need can be met by government. Jackson locates the source of the housing problem in the profit motive: what is most profitable is not the most socially desirable. He does not seem to recognize that there is an equivalent to the profit motive in the public sector—the imperatives of political advantage. In effect, government initiative and intervention are no more a function of unequivocal benevolence and social idealism than

is the activity of the private sector of the economy. Often government programs are more the result of political expedience and calculation than of concern for the public welfare. Just as Jackson's subject is narrowly conceived for the nineteenth century—housing as the study of markets and public legislation rather than life-styles and environments—his dichotomy between the private sector (profit-motivated and antisocial) and government (benevolent) in contemporary life is too simple to be convincing.

ROY LUBOVE
University of Pittsburgh

DOLORES HAYDEN. *Seven American Utopias: The Architecture of Communitarian Socialism, 1790-1975*. Cambridge: MIT Press. 1976. Pp. ix, 401. \$16.95.

This architectural history of seven American utopias focuses on four sectarian groups—Shakers at Hancock, Mass.; Mormons at Nauvoo, Ill.; Perfectionists at Oneida, N.Y.; and the Inspirationists at the Amana Colonies, Iowa—and three secular ones—Associationists at North American Phalanx, N.J.; Union Colonists at Greeley, Colo.; and a Socialist commune at Llano del Rio, Calif., which later relocated at Newllano, La.

Author Dolores Hayden discusses one dimension of communitarianism that has received scant attention, namely the relationship between an intentional society's ideology and the design of its buildings and town sites. The Perfectionists and Associationists, for example, commonly used interior circulation spaces to bring members together, while Amana residents developed a communal path system between kitchens, shops, and dormitories to facilitate group interaction. Shakers, on the other hand, used partitions and other devices to keep followers apart.

Former-day Utopians faced one major architectural dilemma—should they build unique structures or replicable but less spectacular ones? The problem developed when communards planned or actually built a series of colonies at scattered locations. An "intentional vernacular" style of structure emerged—one which utilized local building materials and a few basic designs, all adaptable to site conditions and community needs.

Hayden concludes that extant buildings of historic societies must not be viewed as curiosities. In fact, they have value for today. As she argues, "A new architecture expressing liberation can come only from a liberated group of people, not from an idealistic architect or planner sitting at a drawing board."

Seven American Utopias is marvelous architectural history. The author's ideas are clearly developed and cogently argued. The extensive illustrative

materials further enhance the book's quality. But this reviewer is troubled by both the examples selected and the historical research. At first glance, the seven colonies selected by Hayden appear to afford excellent balance as to type, time period, and geography. As she says, "together they provide a fair representation of the ideological and geographical spread of the communitarian movement between 1790 and 1938." Yet, the author totally ignores one of the most important eras of intentional community formation, the late 1880s and especially the 1890s. (The Greeley colony dates from 1869-71 and Llano from 1914-38.) If Hayden had examined the architectural story of the Colorado Cooperative Company (1894-1905), Freedom (1897-1905), or Home Employment (1894-1905), for example, her overall generalizations might have changed. These communitarians seemingly expressed little or no interest in building or community design, beyond wanting structures that were inexpensive and adequate for basic human needs. Since these three experiments were not trying to duplicate colony sites, the unique versus replicable problem did not concern them.

The quality of historical research varies. Hayden's major weakness is that she at times relies heavily on so-called "standard" works that have been either superseded or significantly modified by new monographic and especially journal literature. And, too, Hayden seems unaware of recent scholarship that has challenged the old notion that communitarian thinking was popular only between 1820 and 1850; perhaps that is why she failed to consider the architecture of the "new communitarianism" of the late 19th century.

H. ROGER GRANT
University of Akron

JAMES W. SANDERS. *The Education of an Urban Minority: Catholics in Chicago, 1833-1965*. (Urban Life in America Series.) New York: Oxford University Press. 1977. Pp. xviii, 278. \$13.95.

James Sanders is convinced that in our modern urban world the school has played a crucial role, socially and culturally as well as simply educationally. And the Roman Catholic educational system has served this purpose for at least one-fourth of American youth in our major cities. Thus, using Chicago as a test case, he studies the development of Catholic education for over a century, in terms of its internal development, its role as a social actor, and its role as reflector of what was going on around it. In so doing he provides valuable insights into all of these related phenomena.

The book is divided into three broad chronological periods: early developments up to the First World War; the rapid change of the 1920s under

George Cardinal Mundelein; and the continued success from the 1930s, which has turned into apparent decline in the past ten years. In all of this he is concerned with the social context and the social ramifications—not simply with institutional growth or curricular change.

Roman Catholic schooling originated in the religious factor, Protestant domination of culture and schooling, and a felt Catholic need for schooling to be part of the supportive religious experience of its members. This need was reinforced by the tremendous immigration at the turn of the century of Catholics who felt culturally as well as theologically strange in America, and who were accustomed to religious incursion in all aspects of life. But immigration also created great problems for the Irish hierarchy. Ethnic particularism was as strong in the area of schooling as it was in parish control. One of Mundelein's greatest charges was an attempted reconciliation of ethnic differences among Catholics.

Poverty, too, was a constant social force. Chicago's Catholics were poor people, and this influenced their attitudes and behavior, and also placed some limits on what a Catholic educational system could be and do.

In the 1920s Mundelein set the basis for the system's great growth and success. Parish control and decentralization of education were replaced with some real coordination; and extra-parochial building, especially of secondary and higher educational institutions, expanded the system and helped overcome ethnic and other types of division.

From the 1930s to the 1960s the Church and its school system both flourished, and served one another. Even the rise of an increasingly large Catholic middle class, and suburbanization, did not impede both quantitative and qualitative growth. Only race, as a new problem, seemed beyond ready solution—and indeed it remains to the present one of the key social and educational problems within the Church.

In a brief conclusion Sanders argues that the rapid decline of this successful system since the 1960s was due primarily to changes in the Church itself. Economic factors, levels of religiosity, and race have also played their part. But it may well be that this central institution in the lives of Chicago's Catholics is now in irremediable decline.

The book is well researched, readable, and a worthy addition to the *Urban Life in America* series. From the standpoints of urban, ethnic, religious, and educational history, it makes a useful contribution.

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HAROLD F. WILLIAMSON and PAYSON S. WILD. *Northwestern University: A History, 1850-1975*. Evanston: Northwestern University Press. 1976. Pp. xiii, 403. \$12.50.

Because of its rapidly expanding role, higher education has been a vital part of the intellectual history of the past hundred years. Its opportunities became available to most of American youth, and the new elective, pluralistic, professional, and graduate training replaced the former prescribed "collegiate way." An American open market for institutions of higher learning had enabled an amazing variety of schools to engage in competitive pursuit of these goals. This history of Northwestern University is a study of one of the more effective types of such institutions. Initiated in 1850 by public-spirited Methodist leaders on the northwestern frontier, it grew into a successful private, urban, secular university.

A team of two authors, an editor, research associates, and other assistants have produced an attractive one-volume synthesis of its fortunes. They are to be congratulated for selecting a chronological organization that acquaints the reader with the multiple facets of a given generation rather than isolating the academic divisions in separate chapters. Marginal headings serve as helpful guides. The factual, well-organized text will be welcomed by loyal alumni and others. The format is excellent.

Each reader will have to decide whether these very commendable attributes were bought at an equitable price. Anything more than "a general account of the major curricular developments" was judged to be outside "the most significant features" of the school's history. The intellectual questings of Academe are therefore not emphasized. Inadequately probed, for example, are liberal education's nature and role, the scientific impact of the "Flexner Period" on medical study, the law school's dual goals of training legal technicians and defining the relationship of law to the democratic process, and the ideological roots of the activist sixties on this and other campuses. Construction of a leading factory of education has been carefully traced, but the peruser does not experience vicariously the excitement of its workers in classroom, library, or laboratory.

In summary, the extent to which the impressive Northwestern model may be followed by others attempting to trace the histories of their own schools will depend on the chosen emphases.

CEDRIC CUMMINS
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HELEN LEFKOWITZ HOROWITZ. *Culture and the City: Cultural Philanthropy in Chicago from the 1880s to 1917*. Lexington: University Press of Kentucky. 1976. Pp. xv, 288. \$14.75.

When the Chicago Academy of Fine Arts opened its doors in 1879 to the public it hoped to serve—not simply to the "few lovers of art"—it inaugurated a new cultural era for the city. After it came a succession of cultural institutions dedicated to inspiring the citizenry with the virtues of high art and spiritualizing the city's society. The movers behind this cultural phenomenon, according to Helen Lefkowitz Horowitz in her thoughtful study of "cultural philanthropy" in Chicago from the late nineteenth century to World War I, constituted an elite group of wealthy merchants, food processors, and financiers, primarily of British descent, New England background, and the Protestant religion, who "loved the arts and letters and . . . saw them as a part of the good life." These men also shared a loyalty to the city at a time when national ties were becoming more important than local. Deprived of political responsibility as a result of the domination of local politics by recently arrived immigrants and fearful of social and economic change as a result of labor upheavals, the "cultural philanthropists" (an unfortunate nomenclature) sought to establish control over the city's life by founding and sustaining cultural institutions. The results were a first-rate orchestra, the Art Institute, the Field Museum of Natural History, numerous important small libraries, and, of course, the University of Chicago.

To Horowitz and the individuals she is concerned with, culture meant the fine arts of the past, viewed in terms of their idealistic function as instruments of refinement and morality. The cultural institutions founded during this era reflected in their policies and programs this definition. They changed only in response to criticism by reformers like Jane Addams and John Dewey and the efforts of young professional administrators to expand their activities to include immigrants and children and to introduce more innovative programs. As artists, poets, and writers became more insistent about receiving a share of the patronage afforded to culture, they broke down the older emphasis on historical art and turned the patrons' attentions toward the living artist, and also toward modernism and a broader definition of culture. With World War I this distinctive period in Chicago's cultural development came to an end, although the older concern for culture continued and led to the founding of new public institutions designed, however, to serve different purposes and fulfill different expectations.

Horowitz's study is more descriptive than analytical, and the absence of a bibliography makes it difficult to assess her total achievement. Her limited concern with the special phenomenon of cultural leadership in Chicago tends to simplify what is really a complicated and dramatic story, involving especially the role of ethnic groups inter-

acting with the native-born in a volatile and fluid urban situation. The book does offer, however, some suggestive insights into the process of urban cultural development and change and especially into the origins, motivations, and influence of elite groups in the Midwest at the turn of the twentieth century.

LILLIAN B. MILLER
National Portrait Gallery,
Smithsonian Institution

JACK S. BLOCKER, JR. *Retreat from Reform: The Prohibition Movement in the United States, 1890-1913*. (Contributions in American History, number 51.) Westport, Conn.: Greenwood Press. 1976. Pp. xii, 261. \$14.95.

Jack S. Blocker has given us a carefully reasoned, tightly controlled monograph on the transformation of the prohibition movement from its abolitionist origins to its triumph with the passage of the Eighteenth Amendment. His analysis of alternative possibilities for the Prohibition Party in response to Populism enriches our understanding of the Prohibitionist Party's pluralism as the narrow and broad gaugers battled over the future of their middle-class politics. For Blocker the retreat from "economic radicalism" or reform on behalf of the lower classes is the key to understanding the limits of middle-class reformers. Their defense against threats from the lower classes and an impersonal corporate economy that seemed to endanger their self-interest turned them to pessimism and anti-socialism. This failure within the Prohibition Party cleared the path for the Anti-Saloon League—in many ways the diametric opposite of the Prohibitionist Party—to gain political initiative as a pressure group and to win allegiance of church groups.

As reasoned as Blocker's use of social class is, he only partially succeeds in his critique of prevailing interpretations about prohibitionism. He indicates that Timberlake's analysis of the Prohibitionist ideology is wrong in substantial aspects but does not precisely explain how. Since Timberlake's *Prohibition and the Progressive Movement* (1963) focused on the years 1900-20, when the substantial debate over Populism was over, Blocker has perhaps overstated his differences with Timberlake. In fact, Timberlake specifically allows that, "although the prohibition movement took on the nature of a conflict between country and city, it is better understood if viewed more as a class than as a rural-urban struggle." Blocker has extended this class analysis. He also undertakes a critique of the plausibility of John J. Rumbarger's hypothesis about the role of John D. Rockefeller and business in the Anti-Saloon League. Although his critique has merit, he does not present the Rumbarger

hypothesis fully enough for my satisfaction. The historian would benefit from a reading of this dissertation since the issues are complex and can not be adequately discussed within the limits of a book review.

Lastly, Blocker's "inner" history—intra-Party battles—would have been better served had he dealt more with the "inner life" of an emerging middle class. For example, Norman H. Clark has incorporated recent social histories on the family, social purity, and psychology in *Deliver Us From Evil: An Interpretation of American Prohibition* (1976), amplifying subjects only touched upon in Blocker's book.

As we move toward a new social history, we find ourselves lacking a systematic interpretation acceptable to large numbers of historians. Although ostensibly a monograph, Blocker's book has contributed to a redefinition of issues within both prohibition studies and the larger sphere of American social history. His scholarship is sound and his caution in generalizing about larger issues in social history is quite understandable. Perhaps what will prove to be most important about this book is that Blocker has reopened the Pollock-Handlin debate over the nature of Populism from a new perspective.

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ARTHUR S. LINK, *et al.*, editors. *The Papers of Woodrow Wilson*. Volume 16, 1905-1907; volume 17, 1907-1908; volume 18, 1908-1909; volume 19, 1909-1910; volume 20, 1910. Princeton: Princeton University Press. 1973-75. Pp. xxii, 598; xxii, 647; xvii, 672; xxii, 785; xxii, 604. Vol. 16, \$20.00; vols. 17-20, \$22.50 each.

The move from the world of scholarship and teaching into the world of elective politics is far from unique to Woodrow Wilson. It was easier when careers were less specialized, when an Edward Everett or a Julius Seelye could leave academic posts for public office without any hint that they were invading an area beyond their expertise. But even in the age of the Ph.D., a Paul Douglas or a Eugene McCarthy has appeared often enough that the professoriate has seemed simply another appropriate background from which a democratic society can draw its leaders.

But few shifts from the academy to the political arena can equal that of Woodrow Wilson in dramatic interest and consequence for world affairs. Documentation of that development gives special unity to these five volumes, beginning with Wilson at the peak of achievement as president of Princeton and ending with his announcement that he would accept a nomination for governor of New

Jersey. In 1905, with Wilson's new preceptorial system making Princeton the best known center of innovation among American colleges and with a promising fund drive about to begin, there seemed no reason to leave unless he was bored with success. But the months traced here brought mounting frustration as two of his most cherished plans—residential quadrangles to counter the cliquishness and anti-intellectualism of the club system and the building of the new graduate college in the midst of the campus as an energizing center for the whole university—came under attack and failed of adoption by the trustees. Contemporaneously, his rising stature as a potential Democratic candidate for high office created the possibility of fulfilling a youthful dream.

That higher education should strongly influence political life had long been a Wilsonian article of faith. It shaped both his address at Princeton's sesquicentennial celebration in 1896 and his inaugural in 1902. His academic ideals of liberal culture and intellectual discipline were linked to a vision of preparing elite leadership for public life. For him, the free inquiry of the university was akin to the free exchange of ideas in Liberal society, and though only a small minority would receive higher education, as long as it functioned well "the Ship of state will not be without pilots" (16: 270).

When, in February 1906, George Harvey, head of Harper's publishing house, urged Wilson as a candidate for the presidency, there were no important pressures to propel him away from the university. Still, he did not go to sleep that night before thanking Harvey for the compliment. When newspaper editorialists and others took up the idea, Wilson showed himself a master of the non-refusal refusal, declaring, "Nothing could be further from my thoughts," yet summarizing the very qualities that made him an attractive candidate (xvi: 330-31). Indeed, Wilson offered a promising alternative for a party that had no luck with Bryan, but seemed to do even worse when it turned away from him. Politically significant traits of Wilson which have been generally forgotten emerge in these volumes. Audiences stirred him up, and speeches based on fairly austere drafts seemed spontaneous and inspiring as delivered. He was a master of timing and of the telling anecdote, often making himself the butt of the joke. One reporter cited Wilson's resemblance to "a typical young American businessman," with a jovial manner and an easy, noisy laugh (16: 116).

This politician hidden under academic robes was quickened by Wilson's first great frustration in academic life. In 1907, after nearly unanimously supporting his plan to institute an inclusive system of residential quadrangles that would absorb the selective eating clubs, the trustees yielded to

alumni pressure and rescinded their vote. A humiliated Wilson drafted part of a letter of resignation. Though he stopped short, it is clear that his attitude toward a political career had shifted. (In spite of his announced noncandidacy, votes had been cast for him when the New Jersey legislature was choosing a Senator in January, 1907.) Now when invitations came to give toasts at Democratic gatherings, Wilson performed with extra zest. He spelled out in some detail an alternative to Bryanism and to the regulatory program of Theodore Roosevelt. Government should not regulate business through commissions or try to correct abuses by fining corporations; rather, individuals guilty of crime should be punished through court processes. "The Democratic principle," as he identified it, was "that no regulation incompatible with the freedom and development of the individual is tolerable" (19: 466). Though thinking of himself as conservative rather than progressive and scorning most proposed changes in electoral procedure, Wilson did advocate the short ballot, seeing it as a way of focusing responsibility.

If more specific credentials were required, Wilson could provide them. Although he belittled systematic writing on politics and claimed he had learned more about politics from poets, he was not averse to reminding audiences that he was a political scientist and a lawyer, too. His last scholarly work, *Constitutional Government in the United States* (1908), originally presented as lectures at Columbia University, contrasts revealingly with his classic *Congressional Government* of 1885. No longer recommending the cabinet system, he pointed to the potential power of the president, who "is at liberty, both in law and conscience, to be as big a man as he can" (18: 116). He wrote insightfully about the unifying function of parties in a political system designed largely to frustrate the use of governmental power, and he had understanding words for the role of party bosses.

The spring of 1910 brought Wilson's second great defeat at Princeton, the adoption of Andrew F. West's plan to build the graduate college at some distance from the main campus and West's retention of power as dean of the Graduate School. Wilson was now ready to encourage Harvey's renewed interest in moving him into Democratic politics. How, he rationalized, could he do otherwise after his many statements to students about public duty? Meeting with party bosses, he assured them of his respect for the Democratic organization and his willingness to consult on appointments. On July 15, 1910, he formally announced his readiness to accept the Democratic nomination for governor.

Other themes besides academic battles and political maneuvering recur in these documents,

among them the lives of women who had Wilson's love and friendship, the strengthening of science in the Princeton curriculum, the uneasiness of universities in the face of the small college's returning popularity, the gradual modification of the preceptorial system, Wilson's sporadic ill health, including unrecognized strokes. The editing of the series remains exemplary. Especially enlightening are the "collateral letters" of trustees, neither to nor from Wilson, but very much about him. Users of the series may well wish for more of the "Editorial Notes," which lend valuable context to the documents on which they bear. Perhaps, too, it would have been a better policy to repeat the identifying footnotes for individuals at the first appearance in each volume, rather than offering an index reference to where such a note can be found. It is not reasonable to assume that every user of a volume has the entire set at hand, and the increase in printing cost would be relatively small.

HUGH HAWKINS
Amherst College

ARTHUR WALWORTH. *America's Moment: 1918, American Diplomacy at the End of World War I*. New York: W. W. Norton and Company. 1977. Pp. vii, 309, \$14.95.

According to Arthur Walworth, the critical "moment" for Wilsonian diplomacy during the World War lasted approximately four months. It began in early October, 1918, when the Central Powers sued for armistice agreements, and it extended to mid-January 1919, when the Paris Peace Conference formally commenced. During this interim, President Wilson sought to bind all the belligerents, victors and vanquished alike, to his program for peace based on the Fourteen Points and subsequent pronouncements. At this time, Wilson, through the moral force of his rhetoric, was riding a crest of popular support. Seemingly, he held important trump cards that would assure the success of his program at the "Sessions of the World." These included America's economic power, the naval building program that promised an American fleet second to none, reliance on competent advice from the Inquiry and other agencies of government, and a measure of moral superiority arising from the disinclination on the part of the United States to exploit the war for national advantage. Wilson could also exercise the threat of disengagement from the victorious allied-American coalition if and when allied statesmen would reject the president's conditions for peace. Here, then, was Wilson's and America's moment to exercise world leadership.

No other scholar has examined this "moment" with the intensity applied by Walworth. His multi-

archival research and reliance on numerous manuscript collections scattered on both sides of the Atlantic are reflected in meticulous and copious footnotes and a marvelous bibliography. Throughout the book, Walworth displays clarity in narrative writing that is not surprising for so experienced a literary hand. Unfortunately, for all the exemplary research, the illuminating insights, an absorbing synthesis, and the yield of much rich detail, the book fails to alter the conventional interpretive structure found in the earlier historiography and certain memoirs of this period. In part, the lack of novel interpretation can be explained by Walworth's concern with essentially the same questions and hypotheses posed by his predecessors. The lack of a central, organizing theme is also apparent. Moreover, Walworth tends to avoid making difficult and sometimes harsh judgments of Wilson, Colonel House, and other principals, preferring to let readers draw their own inferences from the evidence. There seems to be an inconsistency in the portrayal of Wilson, whom Walworth often characterizes as a moral idealist, "the prophet," while much evidence suggests that Wilsonian strategy was predicated on such practical considerations as a new balance of power in post-war Europe. The evidence also constitutes a serious indictment of both Wilson and Colonel House with reference to staffing the American Commission to Negotiate Peace, their failure to plan in advance for the Armistice, and Wilson's delay in drafting suitable, specific plans for the League of Nations prior to the Peace Conference. As for House, he assumed that he had scored a diplomatic triumph at the Supreme War Council in late October and early November 1918, when trying to commit the British, French, and Italian leaders to the Wilsonian principles while negating the brunt of the earlier allied secret treaties. This mistaken reading of events amounted to a form of self-deception that would have serious consequences for the future peace conference. Walworth's treatment of the intricate negotiations leading to the Armistice is a highlight of the volume.

"America's moment" was but prelude to the vastly more complicated peace negotiations. Walworth's book deserves a careful reading by students of Wilsonian diplomacy and of the Paris Peace Conference. I shall look forward to the sequel.

LAWRENCE E. GELFAND
University of Iowa

SAMUEL and DOROTHY ROSENMAN. *Presidential Style: Some Giants and a Pygmy in the White House*. With an introduction by JAMES MACGREGOR BURNS. New York: Harper and Row. 1976. Pp. xx, 602. \$16.95.

This interesting survey of familiar aspects of the presidencies of four "giants"—Theodore Roosevelt, Woodrow Wilson, Franklin D. Roosevelt, and Harry Truman—and one "pygmy"—Warren G. Harding—is valuable mainly for the insight it gives into the values and attitudes of the late Samuel Rosenman, FDR's longtime speech writer. The judge completed the chapters on the two Roosevelts before his death, and though the three others were written largely by his wife, Dorothy Rosenman, there is no perceptible difference in viewpoint. Dorothy Rosenman also did most of the research for the entire book. Based largely on standard biographies and published collections of speeches and writing, the research ignores most of the pertinent monographic literature. In the case of Harding, moreover, the journalistic accounts of Samuel Hopkins Adams and Francis Russell are used to the exclusion of Robert K. Murray's more authoritative study.

As one would expect, the Rosenmans emerge as persuasive advocates of a strong, activist presidency in the model of James MacGregor Burns. They regard Congress as a congeries of factions and special interest groups to be led rather than followed, and they describe with approval those instances in which the "giants" forced it to submit to the national will be mobilizing public opinion in support of presidential programs. They accept the need for deviousness, which they distinguish from deceit, and they applaud constructive compromise. Despite unconcealed admiration for each of the "giants," the authors are not insensitive to their faults; with some exceptions, however, they interpret them charitably. Although the work is schematic, in the sense that it focuses on each president's style, no effort is made to systematize or summarize the mature and often perceptive observations which are scattered throughout the text. In a work in which the chief merit is, to repeat, the light it casts on one of its illustrious authors, this is doubly unfortunate.

WILLIAM H. HARBAUGH
University of Virginia

JAMES E. HEWES, JR. *From Root to McNamara: Army Organization and Administration, 1900-1963*. (Special Studies.) Washington, D.C.: Center of Military History, United States Army. 1975. Pp. xvii, 452. \$11.45.

This study of the structure and management of the War Department and the Department of the Army from the turn of the century through the Kennedy administration is, unlike many administrative histories, stimulating and provocative. An official army historian since 1962, James Hewes demonstrates a mastery of not only modern army organi-

zational changes but also institutional trends in contemporary big business. Indeed, he states that the "framework" for his book came "from Alfred Dupont Chandler, Jr., in his pioneering studies on the development of modern American industrial management."

The army's organizational evolution during these six decades, according to Hewes, was characterized by a continuous struggle over whether executive control should be at the level of the bureaus and technical services or at the level of the chief of staff, secretary of war, and, since 1949, secretary of defense. He identifies two main camps of antagonists: the "traditionalists," consisting mostly of the key staff officers of the bureaus and technical services, who espoused decentralization of management; and the "rationalists," including most of the strong chiefs of staff and secretaries of war, who favored centralized control utilizing modern management methods to ensure regulation and order "from the top down" in curbing the inefficiency and waste of unregulated bureau activities. Hewes depicts the latter group endeavoring to impose on the army an effective centralized power structure similar in many respects to that which some large corporations established. He claims also that "this struggle has reflected a similar one in the American society at large as the nation evolved from a loose-jointed federation into a highly industrialized, urban nation."

Hewes' task was difficult in trying to explain the often complex plans of numerous reorganizational efforts. But he succeeds admirably, writing with clarity and conciseness, analyzing each plan perceptively and objectively, and supporting his narrative with thirty-one organizational charts that, like good maps in an operations study, are essential to the reader's understanding.

His emphasis is on the years 1942-63, with eighty-seven percent of the pagination allotted to developments of that period. Slight treatment (only seven pages) is given to the years 1919-39, but perhaps justifiably.

This volume is the first of a new series by the Center of Military History, called "Special Studies." Future volumes will include Hewes' sequel, now in preparation, on army organization and administration, 1963-74.

D. CLAYTON JAMES
Mississippi State University

STUART EWEN. *Captains of Consciousness: Advertising and the Social Roots of the Consumer Culture*. New York: McGraw-Hill Book Company. 1976. Pp. x, 261. \$10.00.

Toward the close of this study of how American industrial capitalists have tried through advertis-

ing to "control the entire social realm," Stuart Ewen reports that in the 1920s Carl Naether, an expert on advertising directed at women, expressed enthusiasm for an ad featuring an elegantly dressed woman with pearls, who (in Ewen's words) "with one hand . . . fondled her bosom," thus encouraging among women a desirable "self-confidence about their bodies." Ewen offers Naether's advice as one of many pieces of evidence in support of the major argument of his book. To sell their vast surplus production, American corporations in that decade tried through policies of high wages, shortened hours, easy credit, and heavy advertising to call into existence a mass market of compliant consumers, increasingly dependent on commodities for satisfying intangible needs and reducing social frustrations. Advertising attempted to deflect the dissatisfactions of consumers (essentially women) away from the goods themselves and from the system that produced them and inward toward their own persons, and to encourage them to substitute for social and political protest the ever-larger purchase of commodities in a compulsive and futile quest for self-improvement. The consequence, for women in particular, was a narcissistic and competitive preoccupation with sexual attractiveness. In time men and women alike came to look to the corporation and its "captains of consciousness," rather than to family, friends, and community, as the source of values. The result, Ewen argues, has been a depressing and perhaps irreversible psychic loss for American culture: the crippling of the capacity of ordinary people to find pleasure in the intrinsic quality of things, in their personal relations to one another, or in their own integrity and self-respect as persons.

Ewen's contribution to scholarship is difficult to weigh. Despite an unconvincing overlay of Marxism and pages of unbelievably opaque and clumsy prose, his comments on the family in the industrial consumer culture, and the changing role of women within that family, are penetrating and thoughtful, and his interpretations of advertisements are occasionally ingenious. The evidence that he offers in support of some of his generalizations, however, will equally sustain an opposing view. An ad man's enthusiasm, for example, for a photograph that depicts a woman fondling her own bosom and thereby calling favorable attention to the self-confidence that grows from sexual liberation might well have the effect of encouraging in women a greater *independence* from the artificially imposed anxieties of a commodity market. Ironically, it is Ewen's carelessness with sources that in this instance may have saved his fundamental argument. He either failed to see or chose to ignore the fact that the woman in the ad is not fondling her bosom

at all but holding the lining of her cloak; and, equally important, Naether draws for his readers none of the erotic conclusions that Ewen attributes to him. Naether praises the ad because it focuses attention on the pearls and what pearls will do for the person who wears them, and Ewen appears not to have noticed that Naether's actual point supports his thesis more fully than what he misrepresented Naether as saying.

Unfortunately, the "fondled" bosom is not an extreme but a representative instance of the distortion of evidence that plagues Ewen's study. A check of only one third of his sources (but including most of the advertisements that he cites as evidence) revealed at least forty misrepresentations and misinterpretations, and almost as many more errors in citation, transcription, and identification of sources. Some are striking enough to raise doubts not only about some of his arguments but about the seriousness with which he and his editor have taken their professional responsibilities. Perhaps it is just as well, in view of Ewen's handling of historical evidence, that his observations about the interaction of advertising and society remain fundamentally subjective and dogmatic, resistant to proof and disproof, and unaffected by historical evidence in the first place.

OTIS A. PEASE
University of Washington

THOMAS CRIPPS. *Slow Fade to Black: The Negro in American Film, 1900-1942*. New York: Oxford University Press. 1977. Pp. xi, 447. Cloth \$19.95, paper \$5.95.

The Harlem Renaissance never made it to Hollywood—nor, for that matter, to Astoria, Queens, site of the last major East Coast motion picture studio. During the first half-century of movie-making Hollywood depicted blacks, with few exceptions, as savages, slaves, or servants. But when blacks tried to produce their own movies they faced esthetic, cultural, and economic dilemmas similar to those of the Renaissance writers: Was there a "black esthetic"? Could black culture be depicted separately from the context of "white" American values? Would a black audience support their work? In the period before World War II these questions, for movies at least, were answered in the negative. Black leaders finally put pressure on Hollywood, and in the midst of a war against a racist enemy, a new era of black expression on the screen was launched.

This, in summary, is the argument of Thomas Cripps' *Slow Fade to Black: The Negro in American Film, 1900-1942*. Some of the story has been told, in briefer and more impressionistic form, in Daniel J. Leab, *From Sambo to Superspade: The Black Experience*

in *Motion Pictures* (1975) and Donald Bogle, *Toms, Coons, Mulattoes, Mammies and Bucks* (1973), but no previous work has been so thorough and comprehensive in covering the field. Cripps has tried to mention, so it seems, every film with a black theme or black characters made before World War II in the United States, Great Britain, and Europe, and he describes many of them.

Amid such detail the larger issues can easily be blurred. In particular, Cripps seems ambivalent about the relation of Hollywood's images to American racial attitudes. Sometimes he suggests that Hollywood was in the vanguard of change, at other times he stresses the anachronism of Hollywood's antebellum plantation mystique. Although he emphasizes the significance of a 1942 meeting between Walter White of the NAACP and studio heads, out of which came the promise, as *Variety* headlined, of "BETTER BREAKS FOR NEGROES IN H'WOOD," he also gives evidence that a few liberal directors and writers may have been the principal source of improvement. In any case, it takes a bleak past, and a bit of grasping at straws, to see Dooley Wilson's role as Sam in *Casablanca* (1942) as a major transformation of the black image.

More than many historians of mass media images and stereotypes, Cripps is aware of the complexity of their functions. He shows how the image of successful black actors in Hollywood may have been more important to blacks than the stereotyped characters they portrayed. He also shows how difficult it was for independent black filmmakers to provide alternative images that were effective esthetically or commercially.

This book needed a firmer editorial hand. The prose is repetitious, syntactically awkward, occasionally ungrammatical. Cripps has presented the main lines of this work more clearly and concisely through images in an excellent documentary he wrote for television, "Black Shadows on a Silver Screen" (1975).

ROBERT SKLAR
New York City

C. ALLYN RUSSELL. *Voices of American Fundamentalism: Seven Biographical Studies*. Philadelphia: Westminster Press. 1976. Pp. 304. \$15.00.

Our present-day fascination with resurgent fundamentalism is centered more on its best-known personalities, such as Jimmy Carter, Charles Colson, or Eldridge Cleaver, than on its distinctive characteristics. In this revisionist study, C. Allyn Russell takes us back to the origins of that movement and shows convincingly that the cult of personality and celebrity has always characterized fundamentalist leadership and expression. For all its apparent unity in the formative 1920s, a unity historians to this day have accepted, the move-

ment, according to the author, really followed several parallel but decidedly different roads.

To support his revisionist thesis, Russell presents case studies of seven leaders he considers the most influential shapers of fundamentalism. We find thoroughly researched portraits of four Baptists and three Presbyterians: J. Frank Norris, John Roach Straton, W. B. Riley, J. C. Masee, J. Gresham Machen, William Jennings Bryan, and Clarence Macartney. Russell concludes that despite earlier historians' claims that fundamentalists stood united on the "Five Fundamentals," or some similar number, they in fact agreed only on the inerrancy of the Bible and on the supernatural origins and character of Christianity.

Each of the seven leaders is studied in individual chapters. Each chapter includes a brief biography and a summary of the subject's theology, ethics, ecclesiology, and influence on public opinion. While generally sparing in personal interpretation, Russell finds in each person some hitherto neglected quality to praise or some previously unknown dimension to criticize. We learn a good deal more from this study than was previously known about the subjects' attitudes toward the major social questions of the decade. Equally helpful is Russell's acknowledgment that this is not an exhaustive study but rather a preliminary outline of the area to be explored. He suggests such scholarship be based on sociological, quantitative data as well as the "psychological history of the fundamentalist leaders themselves." He includes extensive footnoting and a very helpful bibliography.

Some reservations must be made, however. Would it not have been more fruitful if the author had utilized the possibilities of group biography as William B. Hutchison has suggested (*AHR*, 76 [1971]: 386-411)? A wider sampling of leaders from other denominations, added to these seven, might well change the portrait. Further, since these men were so different in temperament, social position, and most doctrines, one wonders if the use of the term "fundamentalist" to include all of them will be meaningful for future classification? We need a far more sophisticated typology of the anti-liberal, politically conservative, Protestant spokesmen of the 1920s than is presented here.

For the immediate present, however, this work is highly satisfactory. In our current bewilderment over the wide variety of national celebrities who speak as fundamentalists or evangelicals, we can profitably use this study of that movement's origins. We learn that each of those who preached that the way to heaven is straight and narrow was in fact walking down his own straight and narrow road. We now have a better roadmap than before to show us where that road was leading.

ERLING JORSTAD
St. Olaf College

LINDA GORDON. *Woman's Body, Woman's Right: A Social History of Birth Control in America*. New York: Grossmann Publishers. 1976. Pp. xviii, 479. \$12.50.

History was enslaved to politics for this book. Linda Gordon warns the reader that she wrote the book "to argue for my own view of the direction that feminism should take, and my understanding of its history underscores my certainty that I am right. The poses of neutrality or relativism are available only to those who already have power; few women can afford that." Her viewpoint is that of a Marxist-Feminist. The result is a mixture of good and bad.

As the current patter would have it: first the good news. Gordon brings together important material from a range of topics including feminism, socialism, working-class history, psychology, sexology, eugenics, and contraceptive theory and technology; and she weaves it ably to advance a powerful argument supporting her radical feminism. Her "class analysis" reveals an extraordinary complexity for many issues. Since much of women's history has chronicled the efforts and views of the middle-class leadership, Gordon's attention to working-class women and radicals is welcome.

Now, the bad news. The polemical intent of this book makes it untrustworthy. The ideological nature of the argument is thrust upon the reader at many points. Aside from her use of Marxist jargon, Gordon feels impelled to digress to do battle with "vulgar" Marxians and to lecture on the analytical shortcomings of historical figures. Her favorite target is Margaret Sanger, who is repeatedly criticized for failing to do the sort of analysis that Gordon did in writing the book. This is simply antihistorical.

Gordon argues that the movement to win reproductive freedom by women is an effort to revolutionize society and its power relationships. She maintains that the winning of reproductive freedom (birth control) cannot be separated from the class system, capitalist economics, and male-dominated politics and sexual relations. She is critical of those who try to compartmentalize all these and treat them as separate.

The historical heart of this book is the tracing of the movement for reproductive freedom by American women. She shows that the practice of birth control has an ancient history and developed its modern struggle when it was outlawed in the nineteenth century. The crusade had three stages prior to the revival of feminism in the 1960s. In the 1870s feminists advocated "voluntary motherhood," which placed sex within the permanent, monogamous marriage and gave to women the right to decide about conception. However, it rejected contraceptive devices. After 1910 the movement evolved into the socialist phase. Gordon argues

that this made it a mass movement that was truly revolutionary as it attacked the established system of sex, politics, economics, and class. Both socialism and feminism, however, faded in the 1920s. In the 1920s and 1930s professionals—physicians, eugenicists, and social workers—took over and converted birth control into an elite lobbying effort. The epitome of this trust was the creation of Planned Parenthood which was anything but revolutionary or feminist. Planned Parenthood now sought to impose bourgeois values and norms on others. Gordon feels that the birth control movement sold its soul for respectability and fell into the clutches of eugenicists (who were frequently racists) and advocates of "population control" who want to limit the growth of minorities, the poor, and Third World nations.

Like baseball and cricket, history and political polemics have different rules; and it is difficult to judge one by the other. As history, this book is suspect, but as political polemic it is outstanding. It is itself a primary source for the historian of feminism of the 1970s, and is well worth reading.

J. STANLEY LEMONS
Rhode Island College

KENNETH BAXTER RAGSDALE. *Quicksilver: Terlingua and the Chisos Mining Company*. Foreword by JOE B. FRANTZ. College Station: Texas A & M University Press. 1976. Pp. xvii, 327. \$12.50.

If strictly local history has any value beyond feeding the sometimes antiquarian interests of local history buffs, it is when it provides an in-depth case study of some larger, significant phenomenon. *Quicksilver* is an admirable example of such a study. Terlingua was a Texas mining community owned and dominated by a single company which, in turn, was controlled by one man. Hundreds of these one-industry towns have come and gone in the United States, and many have had their stories told in one way or another. But though the well-meaning writers of local histories have created interesting nostalgia, they have not often asked significant questions. One of a handful of well-designed scholarly studies, *Quicksilver* probes the problems and draws significant conclusions. From such specific studies it is possible to test generalizations often made about this larger national phenomenon.

Significantly, nearly all the company town stereotypes were represented in Terlingua. One is the image of the all-powerful absentee owner. Howard E. Perry was a mining industry Napoleon, short in stature but long in self-assurance and the ability to dominate those around him. He had little respect for his Mexican laborers and burst into frequent fits of temper against high-level white employees whom he suspected of incompetence.

Architecturally Terlingua was dominated by the pretentious mansion built for Perry, which stood in stark contrast to the more humble adobe or rock dwellings of the nearly seven hundred Mexican residents. The town boasted a company hotel, a school dominated by a Perry-controlled board, a company doctor, a company-employed justice of the peace, a company-owned telephone line, and a company store. True to the stereotype, Perry not only turned the store into a profit-making venture (during hard times the store was the only profitable enterprise in town), but also tried to coerce employees to trade there exclusively. The company also had its influence in county politics. While the possible abuses in such a system are well documented, Kenneth Ragsdale offers a balanced interpretation that recognizes the imperatives imposed by isolation and the transitory nature of mining.

Perry's mining activities in Terlingua covered nearly forty problem-plagued years. One difficulty arose when the company trespassed into underground ore belonging to another company. The treatment of this incident is especially significant in illustrating the role of secrecy in company operations. Another important chapter discusses the impact of the New Deal, particularly the NRA, as Perry effectively battled federal restrictions and violated the codes of fair practice. Finally, bankruptcy came in 1942, as the ore played out and no rich new bodies were discovered.

One weakness of *Quicksilver* is its sometimes tiresome style, for the reader often tends to get lost in detail. If only it were all written in the superb style of the foreword, prepared by the author's mentor, Joe B. Frantz. On the other hand, Ragsdale's conclusions are well drawn and his epilogue, in which he summarizes his views on Perry, is interesting, direct, and significant. The trouble is that by the time the reader gets to this point he tends to feel complete disdain for Perry. All of a sudden he finds Ragsdale effectively justifying much of what Perry did. More such balance earlier in the text might have helped prepare the way.

JAMES B. ALLEN
Brigham Young University

BARRY D. KARL. *Charles E. Merriam and the Study of Politics*. Chicago: University of Chicago Press. 1974. Pp. xiv, 337. \$15.00.

As a professor, politician, academic entrepreneur, behaviorist, planner, poet, and reformer, Charles E. Merriam was easily one of the outstanding political scientists in the first half of the twentieth century. He was also a progressive with an abiding belief that ideas, theories, proposals, and programs could enhance civic virtue only through em-

pirical testing—and this to Merriam meant political action, the instrument through which moral purpose and broader democracy could be achieved. Planning, a concept to which he was devoted and of which he was a leading advocate, involved determining priorities which experts then would more precisely formulate so that politicians could translate them into reality. Politics was central to the process, and the goal was always a state attuned to the concerns and hopes of its citizenry. Starting with a sense of community, appropriate to the rural Iowa in which he was born, Merriam expanded his horizons to urban America, serving as an alderman in boss-ridden Chicago while gaining scholarly recognition at the University of Chicago, and then to the national scene, helping to organize the Social Science Research Council and the Public Administration Clearing House, among other scholarly enterprises seeking to influence public policy and playing a role in promoting planning during the administrations of two sympathetic presidents, Herbert Hoover and Franklin D. Roosevelt.

Throughout his entire career Merriam was torn by the tension between abstract theory and practical reality, and he always landed on the reality side of the equation, stressing reason and science as forces "for the practical realization of the future," the instruments through which utopias could be translated into meaningful aspects of life, shaping cultural development and enhancing moral responsibility. Concern for planning within this context made Merriam an optimist with an abiding faith in the political process. Barry Karl considers him a heroic figure, as the jacket blurb states, "not only for his specific accomplishments, but because of the enthusiasm and energy he could generate in others towards solving the problems of his time."

But in his last years—Merriam died in 1953—his progressive views were challenged by American scholars who could not accept his optimistic outlook and by European intellectuals who, having fled fascist tyranny, were propounding more extreme views about the nature of man, the deficiencies inherent in political behavior, and the pitfalls of planning in a democratic state. The question of loyalty, the last issue to attract Merriam's attention, might have sorely tested his optimistic outlook had he lived to witness its full flowering during the Eisenhower years.

Karl, who probably conceptualizes better than any historian probing recent American history, has written a splendid intellectual biography. It will help focus scholarly attention on the important topics of planning, professionalization, academic entrepreneurship, and the like. In some of his footnotes Karl indicates that not all scholars

accept his views. And items published after this biography appeared in 1974 confirm this point. Mary Furrer's *Advocacy and Objectivity*, for example, presents a markedly different view of the early academic climate at the University of Chicago than the one Karl presents, while Otis Graham in his recent volume, *Toward a Planned Society: From Roosevelt to Nixon*, barely considers Merriam. Though scholarly debate will challenge many of these themes, Barry Karl's biography, nevertheless, is a notable achievement, one that makes for exciting reading.

RICHARD LOWITT
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W. A. SWANBERG. *Norman Thomas: The Last Idealist*. New York: Charles Scribner's Sons. 1976. Pp. xii, 528. \$14.95.

Like W. A. Swanberg's other works, this biography of Norman Thomas is eminently readable and interesting because of the abundance of well-turned phrases and colorful anecdotes. These do not, however, compensate for the book's lack of analysis, and its endless stream of detail, sometimes trivial.

Scholars seeking answers to questions concerning socialist theory in the United States will have to look elsewhere. Perhaps this reflects more on the subject than the author. As Swanberg informs us, Thomas read P. G. Wodehouse with greater ardor than he ever studied Marx. Motivated more by moralism, pacifism, and a sense of justice and fair play than scientific materialism, Thomas rose quickly to leadership in the American Socialist Party. Considering the circumstances of socialism in the United States, this ascendancy was not illogical. When he joined the party shortly after World War I, the native-born, Princeton-educated, social gospel minister had special appeal and value for the "godless foreigners" of the American Left. He offered the legitimacy and appearance of safety that men with names like Hillquit, Waldman, Cahan, and Vladeck could not. As Vladeck pointed out, any Rotarian could understand Thomas. Unfortunately for the Socialists, as the *New York Times* editorialized on Thomas' sixty-fifth birthday, few men in American public life commanded greater esteem or fewer votes than Norman Thomas.

Swanberg chronicles this public as well as private life year by year. In fact, chronology serves as the book's chief organizing principle to the extent that every other page is headed by the year under discussion. This results in neighboring paragraphs which discuss major policy stands and hernia operations on an equal footing. It also allows the reader to follow all of Thomas' election

campaigns—not simply the six for president but races like the one for New York City alderman in 1927 which preceded the ill-fated contest against Al Smith and Herbert Hoover the following year. In a one-man-bahd, the single individual must play all parts no matter how small. In the Socialist Party, where educating voters could be as important as winning elections, no office seemed too insignificant. If one wonders, however, why the Socialists failed so often, Swanberg offers few direct answers.

While he claims to paint a portrait of "the last idealist," the author also portrays a man of paradox. For all of his empathy and sympathy for the working man, Thomas ever remained the Princeton patrician living relatively comfortably with his wife of social standing and inherited wealth until her death; for all of his support for civil rights, he swam at a club inhospitable to Negroes; his support for civil liberties did not prevent him from advocating the purge of Communists from the ACLU and opposing their teaching in the public schools; his opposition to U.S. imperialism did not alert him to the web of CIA-financed organizations in which he became entangled late in life. Unquestionably, Thomas was an honorable man and, given the measure of other U.S. politicians, even a great man. He was, however, not a saint, a fact which he would probably admit with the same sense of humor which permitted him in old age to hobble to a lectern and intone to his audience, "Creeping Socialism!"

The biography is based primarily on the collection of Norman Thomas Papers at the New York Public Library and on newspapers and interviews. Swanberg did not use the extensive Socialist collection at Duke University, which would have offered him greater insight into the factionalism within the party, especially during the 1930s, and into the mechanics and theory of U.S. socialism generally. He does not claim his work to be definitive; it will, nevertheless, effectively complement the earlier studies of Seidler, Fleischman, and Johnpoll. This book should prove popular with the general reading public and useful to scholars, who will await more analytical studies of this man who probably desired to be president as well as right.

BRUCE M. STAVE
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ALFRED E. ECKES, JR. *A Search for Solvency: Bretton Woods and the International Monetary System, 1941-1971*. Austin: University of Texas Press. 1975. Pp. xiii, 355. \$10.00.

Although the subtitle suggests something much broader, *A Search for Solvency* focuses on the creation

of the International Monetary Fund and the World Bank. After briefly surveying the breakdown of the international economy in the 1930s, the author devotes more than two-thirds of the book to the Roosevelt administration's efforts, culminating at Bretton Woods in July 1944, to promote a new economic order by establishing international institutions to maintain stable exchange rates and to finance development. An extended epilogue covers the operation of the fund and bank up to Nixon's New Economic Policy of 1971, "the end of the Bretton Woods' era."

Alfred E. Eckes' study is based on a wide variety of published and unpublished sources, and provides a valuable analysis of this neglected aspect of American foreign policy in World War II. The author skillfully relates postwar planning to contemporary economic thought, and documents the important role played by an international elite of professional economists. He shows how the design originally conceived by Harry Dexter White was modified by domestic pressures and in negotiations with other nations, particularly Great Britain. In an especially fascinating chapter, he details how the Treasury Department, with the help of public relations experts, mounted one of the "most elaborate and sophisticated campaigns ever conducted in support of legislation" to sell Bretton Woods to Congress.

Eckes vigorously challenges those historians who have viewed American economic foreign policy as essentially exploitative. He concedes that the "founding fathers" of the new order were motivated by national self-interest and that they created institutions that would be subject to United States' control. But, he argues, their goal was a "mutually beneficial economic order" rather than hegemony, and they were sensitive to other nations and willing to make concessions to meet their needs. He concludes that while the fund and bank suffered from structural weaknesses and never played the central role their founders had envisioned, Bretton Woods was a "landmark in global economic history" and helped to promote an era of unprecedented international prosperity.

The epilogue is necessarily superficial, and the title promises more than the book delivers (or even attempts). Nevertheless, *A Search for Solvency* is an excellent monograph which adds an important dimension to the study of American foreign policy and politics during World War II.

GEORGE C. HERRING
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JOSEPH P. LASH. *Roosevelt and Churchill, 1939-1941: The Partnership That Saved the West*. New York: W. W. Norton and Company. 1976. Pp. 528. \$12.95.

Franklin D. Roosevelt continues to puzzle, fascinate, and occupy vast numbers of historians and readers. As a public leader who performed what seemed at the time to be political miracles, it is only fitting that he should have gone through so many miraculous transformations since his death. The post-declassification deluge of monographs on World War II (which is only just beginning) promises a re-interpretation of Franklin Roosevelt's foreign policy, but then such re-interpretations are *de rigueur*. The most long lasting and controversial of these is the "he led us into war" approach, championed by Charles Beard—who accused Roosevelt of deserting domestic reform for foreign adventurism; and echoed on the right by Charles Tansill and those who condemned Roosevelt's domestic policies before moving into foreign affairs. Roosevelt's defenders (epitomized by Basil Rauch) argued that the president was forced into a confrontation with Germany and Japan by the policies of those two nations. William Langer and S. Everett Gleason provided an array of detail on the steps toward war, but moved inexorably toward the same conclusion—that war was forced on America.

Joseph Lash's study of American foreign policy from the invasion of Poland to Pearl Harbor begins with the Churchill-Roosevelt exchanges and fills out the picture with solid research in both British and American archives. Lash may well be the best narrative historian since Allan Nevins. Except for an occasional flowery metaphor, his style is clean and neat, and, like Nevins, he brings a clarity and simplicity to complicated events. Unlike so many historians of World War II, Lash (again like Nevins) has avoided trivia and meaningless vignettes, limiting his anecdotes and storytelling to those episodes which illuminate rather than merely entertain. This is a beautifully written book which captures the spirit of the two leaders and is well worth the time spent reading it.

Lash offers some thoughtful insights into the personalities of both Churchill and Roosevelt as well as some perceptive comments on their relationship. His picture is clear; Roosevelt and Churchill, for all of their faults, were the great men which the times demanded. They occasionally suspected each others motives—Lash makes much of Roosevelt's initial distrust of Churchill's conservatism, imperialism, and anti-Bolshevism—but by 1941 had developed a remarkable degree of openness and candor in their dealings. Churchill was the inspirational leader who could turn military defeat into rhetorical victory. Roosevelt was the "fox" who knew where he was heading, though he was unsure as to just how to get there. As in other recent interpretations (e.g. Sherwin and Bernstein on atomic bomb policy), Lash sees Roosevelt as a man with a consistent and clearcut

goal—in this case, intervention. That conclusion is inductive. Nowhere does Roosevelt proclaim intervention as his goal, yet Lash relates action after action which steadily moved America in that direction. Beard *et al.*, claimed just that many years ago, but Lash has added substance to those theories. Essentially, Lash admits to the conspiracy thesis but then justifies it as the right war at the right time against the right enemy. In Lash's words: "There is . . . little doubt that in the autumn of 1941 Roosevelt was seeking to provoke an undeclared war with Germany because he knew that he could not get a declaration of war from Congress. We leave it to the reader to judge whether he was right. I think he was" (p. 10).

Lash also argues "that at no point did he [Roosevelt] move without having public opinion with him" (p. 10), which brings us to another perplexing debate. Historical public opinion is a vague, amorphous thing which defies precise measurement. Yet, as Lash demonstrates, American public opinion as perceived by Roosevelt steadily supported the broad thrust of the president's policies in the prewar period. As with Vietnam, the American public shared the government's perceptions of the international scene and thus shared the blame—or credit—for American participation. Whether or not public opinion was "educated" or "manipulated" is another question.

Persuasive as the narrative is, its interpretations are somehow unsatisfying. Lash tells us the how and the what, but not the why. After reading the book it is hard to remember that Roosevelt and the bulk of his advisors had committed the United States to a firm anti-German policy before Winston Churchill became prime minister. Moreover, Churchill's role in such commitments as the Charlottesville speech and the Destroyer-Bases Agreement was inconsequential. Events, particularly the fall of France, were the prime determinants of American reactions, since broad policy had already been established. One looks in vain for Churchill to play an indispensable role in shaping American policy from 1939 through 1941, particularly in the Pacific where America's war began. He affected the style and timing of Roosevelt's decisions, but events and American perceptions determined the substance. Lash tends to take Churchill's statements at face value, something the prime minister's advisors never did. There is no question that Roosevelt and Churchill achieved a remarkably close relationship, though Lash correctly notes that a number of underlying issues—from colonialism to postwar trade—were largely avoided lest friction develop. But to call that association a "partnership" stretches the meaning of the word. Not only does it imply an equality that did not exist, but it also overemphasizes the impact of the two leaders on the development of the

Anglo-American Alliance. Moreover, Lash ascribes a firmness and certainty to Roosevelt which makes me uneasy. Henry Stimson said it best when he noted that following Roosevelt's mind was "very much like chasing a vagrant beam of sunshine around a vacant room." To turn Franklin Roosevelt into a man who unwaveringly pursued a single policy goal is too much at variance with the testimony of contemporaries. Roosevelt may have been underrated as a tactician in foreign affairs, but this overrates him as a strategist.

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JOHN B. LUNDSTROM. *The First South Pacific Campaign: Pacific Fleet Strategy, December 1941–June 1942*. Annapolis: Naval Institute Press. 1976. Pp. xix, 240. \$14.50.

This book is intended, says John Lundstrom, "as a strategic overview of the first seven months of the war in the Pacific as it concerned the high commands and fleet commanders of the navies of the United States and Japan." The author has made extensive use of a new Japanese history of early Pacific War operations; of the CinCPac *Greybook*, a war diary kept at Pearl Harbor; and of the CinCPac classified message files, which contain coded Japanese messages deciphered in English. These sources have enabled him to provide a wealth of fresh detail, but he presents only one major reinterpretation, and that is open to question.

According to Lundstrom, Admiral Nimitz in late April 1942 welcomed the impending Japanese seaborne assault on Port Moresby, and the resulting Battle of the Coral Sea, as an opportunity to sink enemy aircraft carriers. This assumption seems improbable because Japan had eleven carriers, and Nimitz had only two available to oppose whatever number the Japanese chose to send.

The traditional view is that Nimitz dared not risk heavy losses in the Coral Sea battle lest he be helpless to meet a later Japanese attack on Midway. Lundstrom thinks this view implies too much knowledge regarding enemy intentions. "Nimitz," he says, "did not learn of the impending Midway offensive until several days after the Battle of the Coral Sea." In fact, Nimitz's intelligence staff, basing its estimate chiefly on cryptanalysis of intercepted radio messages, had predicted in mid-April that the Japanese would attack first Port Moresby and then Midway. Several days *before* the Battle of the Coral Sea, Nimitz and members of his staff visited Midway and thoroughly inspected the atoll's defenses. To the commander of the Midway ground forces, Nimitz said, "If I get you all these things you say you need, then can you hold Midway against a major amphibious assault?"

Lundstrom does not mention the prediction, and disposes of Nimitz's Midway visit in half a sentence that does not include his question.

Written in a deliberately austere and colorless style and somewhat carelessly composed and organized, this book is not likely to attract the casual reader, but it is a valuable reference book for the student of World War II.

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Foreign Relations of the United States, 1950. Volume 2, The United Nations; The Western Hemisphere. (Department of State Publication, number 8853). Washington, D.C.: Government Printing Office. 1976. Pp. xii, 1088. \$13.00.

It is always interesting to examine the *Foreign Relations of the United States* volumes describing events of over a quarter of a century ago, and the U.S. response to them, for it improves one's critical perspective. Over half of this volume concerns the United Nations. Ranked in order of coverage the major topics are: 1.) the question of Chinese representation; 2.) the Trust Territories and Trusteeship Council; 3.) human rights; 4.) strengthening the UN *vis-à-vis* aggression and the Uniting for Peace resolution; and 5.) Secretary-General Lie's peace plan.

In view of the Nixon administration's criticisms of the UN, it is worth noting the concern of the State Department at the time over the disillusionment of certain sectors of public opinion and the Congress, which prompted it to stress the importance of the UN in U.S. foreign policy. For example, the State Department stressed the UN's "centrality" in U.S. policy (pp. 4-5) and the importance of maintaining its institutional structure. The UN was described as a "means to an end," and it was said to be in the U.S. interest to "preserve this means" (p. 30). The U.S. opposed proposals to reorganize the UN without the Soviet bloc countries, for such a reorganization "would probably destroy" the UN without providing a useful alternative (p. 34). The U.S. also stressed the "binding force of international law" (p. 43). Of interest in the 1970s is the agreement by the Committee on Contributions to reduce the U.S. quota almost 1%—from 39.79% to 38.92%—while the U.S. was pressing for 33 1/3%.

A well-known Cold War preoccupation of the U.S. was preventing the seating of "Communist China" due to its involvement in Korea, which prompted Soviet walkouts and boycotts (pp. 186-302). Also as a part of the UN effort in Korea, the U.S. led the drive to circumvent the Soviet veto in the Security Council by turning to the General Assembly upon the basis of the Uniting for Peace resolution (pp. 303-70).

The U.S. record on human rights was generally positive in 1950. The U.S. was critical of the failure of Bulgaria, Hungary, and Rumania to comply with the human rights provisions of the peace treaties and supported the implementation of the International Court of Justice's advisory opinion that the Union of South Africa sign a trusteeship agreement concerning Southwest Africa (now Namibia). During consideration of the International Covenant on Human Rights the U.S. argued for excluding articles on economic, social, and cultural rights and favored their inclusion in a separate protocol. The U.S. was also concerned about including an article for countries having a federal system so that it would "make it possible for federal states to ratify" (p. 512).

The section of the volume on the Western Hemisphere reflects the U.S. preoccupation with Communism, especially in Guatemala. Of considerable interest is a secret memo prepared by George Kennan, Counselor of the Department (pp. 598-624). He observed that Latin American "Communists" were different from their European counterparts and that their "bond with Moscow is tenuous and indirect" on account of the Latin American character of "individualism," "indiscipline," and "personalized, rather than doctrinaire, approach." Kennan made additional observations: a Communist takeover in one Latin American state would not pose "a serious military threat"; the U.S. had given up its right to intervene militarily and had accepted inter-American restrictions; and the U.S. had carried multilateralism too far, at the expense of its bilateral capacity in the security and economic assistance fields (pp. 603, 608, 619-20).

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GEORGE B. KISTIAKOWSKY. *A Scientist at the White House: The Private Diary of President Eisenhower's Special Assistant for Science and Technology.* Introduction by CHARLES S. MAIER. Cambridge, Mass.: Harvard University Press. 1976. Pp. lxvii, 448. \$15.00.

A Scientist at the White House is a practically daily account by George Kistiakowsky of his service as President Eisenhower's assistant for science and technology from spring 1959 to January 1961. In that capacity Kistiakowsky was deeply involved in the test-ban negotiations at Geneva, as well as in helping to shape the nation's satellite and missile programs.

The authenticity of this diary accounts for both its strengths and its weaknesses. Kistiakowsky's personal confessions reflect the essential humanness of persons in high places (for example, his admissions of having dozed off in one important meeting and, on another occasion, having mistaken HEW Secretary Arthur Flemming for Bud-

get Director Maurice Stans). His thumbnail portraits of administration figures are deftly drawn (Richard Nixon emerges as "highly intelligent though calculatingly cordial," p. 190). There is excellent coverage of inter-service haggling, rivalries between the Defense Department and "the President's scientists," and the budget-making process. Moreover, Kistiakowsky has inserted helpful editorial notes, which both identify individuals and summarize subsequent developments in programs alluded to in the diary. Yet the diary format is ultimately frustrating; the subject matter is so complex that topical organization is necessary. So well done are Kistiakowsky's brief chapter introductions (for example, his summary of the late-1950s missile program) that one wishes he had used his diary to write a narrative memoir. Such a work would have been easier to digest, as well as entertaining and informative.

There are frequent insights into Eisenhower's character and attitudes. The diary provides evidence that Eisenhower was an assertive leader who readily listened to conflicting advice and was not as zealous a budget-cutter as is usually believed (Kistiakowsky shows the president frequently calling down Stans and Treasury Secretary Robert Anderson for advocating "penny-pinching" at the expense of "national objectives").

Kistiakowsky's diary is discreet, maintaining silence on "classified projects" and omitting, for example, any retrospective additions to his sparse coverage of the debate within the administration during the tense early days of the U-2 crisis. With the reservations noted, however, this is an interesting and useful addition to the documentation of the Eisenhower years.

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STEVEN F. LAWSON. *Black Ballots: Voting Rights in the South, 1944-1969*. (Contemporary American History Series.) New York: Columbia University Press. 1976. Pp. xii, 474. Cloth \$20.00, paper \$6.95.

The struggle for voting rights has been the most successful aspect of the civil rights movement. Even today, integration in public schools is still piecemeal, segregation remains the rule in housing, and equality of economic opportunity and law enforcement has not been fully achieved. Yet the right to register and vote freely is a reality for black Americans in all sections of the country. This is the consequence of a struggle that began decades ago but bore substantial fruit only after 1944 when the Supreme Court outlawed the white primary. Thereafter, as a result of strenuous and continued campaigns by voting rights groups, the number of blacks eligible to vote in the South rose steadily though unevenly until the Civil Rights Act of 1965

removed the remaining obstacles to registration. The result of that act was a spectacular increase in black (and white) registrants across the South, though the proportion of blacks who are registered has never matched that of whites.

In *Black Ballots*, Steven F. Lawson recounts the political history of this difficult but ultimately successful struggle. The major contours of that history are familiar, and Lawson has not altered them. But he has brought together for the first time the separate threads of the story and woven them into a coherent, informative narrative. His narrative is based on extensive research in the presidential libraries, the records of major civil rights groups, and oral history archives, and on interviews with a number of major participants. No account of such recent events can be definitive until a fuller evaluation of their long range consequences is possible, but for the time being Lawson's book will serve quite well as a general survey of the subject.

Lawson focuses on politics. The lion's share of his attention is thus given to such things as the formulation, passage, and enforcement of voting rights laws and the pressures and counterpressures upon the federal government of voting rights groups and their opponents. None of his data or conclusions is startling, and his concern is to narrate events rather than probe motives or assess the relative weight of the social forces that influenced those events. But his narrative is full of fresh detail, especially on the relationship between voting rights activists and federal officials. Lawson documents the timidity of white moderates in and out of the government, the caution of many liberals, and the difficulties facing blacks who sought to register in the South. He also documents the adroitness of white southerners in manipulating Congress and other political institutions, and the effectiveness of their alliance with conservative northern Republicans. His account of the legislative history of the Civil Rights Act of 1957, perhaps the most important section of the book, is especially illuminating in these respects. Throughout, Lawson's language is readable and his judgments restrained, and the result is a useful book.

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CANADA

JOSEPH P. DONNELLY. *Jean de Brébeuf, 1593-1649*. Chicago: Loyola University Press. 1976. Pp. xii, 346. \$5.95.

Joseph Donnelly's *Jean de Brébeuf* is not only a well-written biography, thoroughly documented and judiciously balanced, but also a reliable account of the Huronia experiment of the Jesuit missionaries

and of the Five Nations war against the northern Iroquoian tribes, especially the Huron. The severest criticism one can offer of this restrained and sensitive account of an heroic life is that the author has failed to underscore sufficiently the fact that the introduction of French trade and of French Renaissance Catholicism (as opposed to Christianity in a more general sense) resulted in just as cataclysmic a revolution in the Huron ancient belief system and life-style as in those of the Iroquois tribes. The natural tendency, given the French and Jesuit nature of the primary sources, to see the reverses, obstacles, resistance and counter-innovative techniques as they were seen and interpreted by the Jesuit participants has not been overcome to the point of attaining complete objectivity. On the other hand, the author's ability to enter into the spiritual vision and conceptual frameworks of the seventeenth-century missionary results in a convincing, well-integrated, and often very moving account.

Brébeuf has not lacked biographers, yet this study clearly surpasses the earlier works of E. J. Devine, Félix Martin, Paul Piron, Joseph Robinne, and Francis Talbot. Generally, the earlier works overemphasized the mortifications of missionary labor and the cruel martyrdom which crowned it. Also, there had been a tendency to dwell unduly on Brébeuf's exalted mystical life and celestial visitations, experiences he shared with a wide range of religious and laymen and lay women who were involved in establishing France's claims to North America in the early seventeenth century. Brébeuf and his companions, to be understood, must be seen in the context of the Catholic revivalism which swept France during these decades and which was best exemplified in the semi-secret cell organization of *dévots* known as the Company of the Holy Sacrament of the Altar.

It is no exaggeration to say that most of what was written in the nineteenth century about the pious founders and missionary clergy of New France was intended (some perhaps specifically commissioned) to document the prerequisites laid down by Rome for beatification and canonization. This biography, of course, plays no such role in the spiritual elevation of a folk hero; nevertheless, its historical reconstruction would support such a cause were it only now being presented to the Congregation of Rites.

Some historians may feel that the concluding chapter on canonization, notably the final sentence, is somewhat incongruous. On the other hand, it could be argued that a more documented treatment of the complicated process of beatification and canonization, together with an historical assessment of the evidence produced, would have been both useful and illuminating.

This paperbound edition is attractively presented with clear print, informative footnoting, relevant quotations from primary sources, useful maps, and excellent bibliography and index. A slight quibble about a bibliographical item may be in order: Alfonsi Carinci's *Acta canonizationis* belongs among the printed primary sources. It is surprising that none of the three published Sainte-Marie Prize books appears in the bibliography, especially Conrad Heidenreich's *Huronnia*—perhaps it was not yet available when Donnelly submitted his manuscript to the publisher.

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LATIN AMERICA

JAMES M. MALLOY, editor. *Authoritarianism and Corporatism in Latin America*. (Pitt Latin America Series.) Pittsburgh: University of Pittsburgh Press. 1977. Pp. x, 549. \$5.95.

This collection of sixteen essays is the outgrowth of a conference on authoritarianism and corporatism in Latin America held at the University of Pittsburgh in 1974. These essays are organized into four sections. The first, by James Malloy, comprises an introductory synthesis of the topic. The second consists of three theoretical pieces, by Douglas A. Chalmers, Guillermo A. O'Donnell, and Simon Schwartzman. The third combines ten case studies: corporatism, clientelism, and partisan conflict in seven nations by Robert Kaufman; success and failure of stabilization programs in Argentina, Mexico, and Brazil by Thomas Skidmore; business and public policy in Mexico by John and Susan Purcell; Mexico's PRI by Evelyn Stevens; the pluralist and corporatist dimensions of interest representation in Colombia by John Bailey; corporatist control of the working class in Brazil since 1964 by Kenneth Mericle; peasant movements in the Dominican Republic by Kenneth Sharpe; authoritarianism in Spanish America by David Scott Palmer; bureaucratic demand-making and clientelistic participation in Peru by Henry Dietz; and authoritarianism and corporatism in Bolivia by James Malloy. The final section includes an overview of Latin American corporatism by David and Ruth Collier as well as a critical examination of the essays in this volume by Silvio Duncan Baretta and Helen Douglass.

In his introduction, James Malloy reminds us of the rethinking on Latin America which has affected both Marxist and liberal democratic theoretical premises. He refers to Juan Perón and Getúlio Vargas as representatives of populist

authoritarianism, to the regime in Peru since 1968 as an example of military populism, and to Brazil since 1964 as a reflection of bureaucratic authoritarianism. In acknowledging these differences he also notes a similarity: "each of these regimes is characterized by strong and relatively autonomous governmental structures that seek to impose on the society a system of interest representation based on enforced limited pluralism." Corporatism evolves from this limited pluralism, and the implication is that corporatism represents some sort of third path between Marxism and liberal democracy.

Whatever the definition of corporatism, however, most of the authors in this volume employ a liberal-democratic framework in their analysis and interpretation. Chalmers derives his discussion of limited pluralism from Juan Linz's analysis of Franco Spain and employs "ideal types" in characterizing politicization in Latin America. Schwartzman explicitly roots his analysis of corporatism and patrimonialism in the writings and thought of Max Weber. Expressing skepticism about corporatist-authoritarian models, Kaufman turns to competitive party groupings as essential forces in shaping corporatist and clientelist patterns in Latin America. Stevens sees in Mexico's PRI an "incomplete kind of corporatism" and looks to other groups such as the Church, military, business, and industry, which are "accommodated within the framework of interaction." For Bailey, pluralism and corporatism are fundamental to interest representation in Colombia. Dietz emphasizes demand-making in a participatory politics in Peru.

Clearly a liberal-democratic thrust pervades these essays. The enterprise attempts to move in new directions, yet its assumptions are drawn from the liberal traditions of North American social science. Corporatism and authoritarianism are viewed as aberrations in a liberal model of pluralism and competing representative groups. In this regard, the volume lacks a balance of contrasting theoretical views.

Only O'Donnell attempts to present an alternative view. He attacks the pluralist, structural functionalist, and systems orientations of the North American academic literature and in an imaginative essay examines corporatism in the context of the bureaucratic-authoritarian state and the classes allied to it. This focus on authoritarianism and the state increasingly is of interest to radical scholars of Latin America. Such scholars have begun to expose the ideological underpinnings of orthodox North American interpretations and to root their analyses in a historical materialist conception with attention to modes of production and class relations.

Awareness of such paradigmatic differences may be helpful to the student who reads this volume. Whatever one's perspective, however, these essays are a welcome addition to the literature on Latin America; they are comprehensive and interesting, scholarly and generally well written.

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ARTHUR J. O. ANDERSON *et al.*, editors and translators. *Beyond the Codices: The Nahuatl View of Colonial Mexico*. With a linguistic essay by RONALD W. LANGACKER. (UCLA Latin American Studies Series, number 27.) Berkeley and Los Angeles: University of California Press. 1976. Pp. ix, 235. \$12.00.

Students exploring libraries and the archives in the capital and provinces of Mexico long have been aware that they contain substantial masses of colonial records in the native Indian languages reduced to Roman script. Frequently the records are accompanied by translations into Spanish, especially if they were presented to a Spanish tribunal. A relatively small number of these records in the most widely used Indian language, Nahuatl, have been put to use in recent decades, most notably by Pedro Carrasco and Edward Calnek, for further evidence on social and political structures, urban and land forms, and the nature of the economy in pre-Conquest times. Except for biographical and genealogical detail, almost nothing has been done with the records for the history of the colonial period itself. This book by Arthur J. O. Anderson, Frances Berdan, and James Lockhart is an eloquent brief that documents in Nahuatl add dimensions indispensable to the study of Indians and Indian society in the colonial period.

The authors present their plea in a series of essays constituting the formal introduction. That, in turn, is supplemented by an essay on colonial changes in Nahuatl linguistic structure and vocabulary by Ronald W. Langacker and by an appendix on further types of records not sampled in the documentary section. The documents, almost entirely selected from the McAfee Collection at the Los Angeles campus of the University of California and from volume 1735 of the *ramo* of Tierras in the Mexican National Archive, are the illustrative material. Almost all relate to Coyoacán and neighboring towns. The authors maintain that through the study of such documents we obtain a more intimate view of the individual Indian and the internal Indian world, one not to be found as easily or perhaps at all in Spanish documents, since the latter reflect, as they must, the approach and interests of the conquerors. The records in Nahuatl are

much more oriented toward the life of the lower sectors of Indian society, but still do not mirror accurately the lowest groups nor show the full range of Indian activity.

Many of the documents deal with land, described in remarkably exact, detailed measurements that are uniformly characteristic of all such Indian documents. The low monetary value placed on land confirms the view that land was relatively abundant in the earlier colonial period. As in any contemporary society, a large proportion of the documents deals with inheritance, political administration at the municipal level, tribute, and taxation. Spanish influence is prominent in the forms of the documents, often derived from Spanish models but reflecting the oratorical approach of Indian culture. Equally apparent is the spread of elements of European material culture, evident also in changes in the Indian language. One of the unexpected impressions that emerges is the extent of movement by Indians within their towns and to neighboring towns. The documents picture a complex society interacting at many levels with Spanish society, adjusting to its conquerors but retaining many elements of its own culture and previous organization of life.

There can be no doubt that the authors have made their case. By extension, records in the other major Indian languages should be of comparable importance.

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ROBERT L. BRUNHOUSE. *Frans Blom, Maya Explorer*. Albuquerque: University of New Mexico Press. 1976. Pp. xi, 291. \$10.00.

Frans Blom was one of the pioneer archeologists in the study of the Maya of southern Mexico. His main contribution was in locating and mapping of numerous sites. For a while he was director of the Middle American Research Institute at Tulane University. As a result of personal problems he lost that position and was no longer in the mainstream of archeological research. In later years he established a research center and home in San Cristóbal de las Casas, Chiapas. He named this center Na Bolom, which is a sort of pun on his name. In the local Mayan dialect it means "House of the Jaguar." This became the meeting place of various anthropologists and a center for some research projects of universities operating in the area.

Blom was a colorful figure of the old school of "explorers," as contrasted with academic scholars. He spent one brief period at Harvard University working toward a graduate degree, but the main influence on his life was exploring the tropical

forest and mountains of Chiapas. His life was interesting, colorful, tragic, and he made significant contributions to our knowledge of the prehistory of southern Mexico. As such, his life is worthy of being commemorated in a biography.

This book by Robert Brunhouse is the fourth of a series by that author dealing with people who have specialized in Mayan archeology. Brunhouse has obviously carefully researched the life of Blom utilizing letters, published records, and interviews with contemporaries, especially Gertrude DUBY Blom who still lives at Na Bolom. One gets a good feeling of the frontier nature of southern Mexico in the early part of this century. The strength of Blom's personality comes through the printed page. The excitement of a new expedition pushing into the jungles is well presented, as are the problems of being an administrator of a growing research program at a university. Many people who worked with Blom are described, and names familiar to practicing archeologists appear throughout the book.

The author is particularly prone to repetition. On one page (193), for example, all five paragraphs begin with a statement about Blom's renewed interests in maps and mapping. If Brunhouse continues his series of biographies, it is to be hoped that this distracting stylistic bent will be corrected.

MELVIN L. FOWLER
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B. W. HIGMAN. *Slave Population and Economy in Jamaica, 1807–1834*. New York: Cambridge University Press. 1976. Pp. vii, 327. \$32.50.

B. W. Higman has produced a comprehensive demographic analysis of the Jamaican slave population between the end of the British slave trade and the abolition of slavery. By applying sophisticated statistical techniques to two major documentary sources—Accounts Produce (a record of all items produced for sale on absentee properties) and the Returns of Registrations of Slaves—Higman has provided new clarity on the diversity and distribution of agricultural property and on the spatial distribution, age structure, and sex ratio of the Jamaican slave population.

Abolitionists considered the absolute decline of slave numbers to be manifest evidence that planters overworked, underfed, and abused their slaves. Higman declares that this cannot be proven, but he shows that mortality was highest on sugar estates and lowest on non-sugar properties producing more than one product. High sex ratios did not depress fertility, and there is little evidence that a high ratio of Africans to Creoles impaired repro-

duction. Climate and environment were negligible factors in slave mortality. The proposition that natural increase in West Indian populations would occur when the peculiar age/sex structure produced by the slave trade worked itself out is not easily verified by Jamaican statistics. In Jamaica, variations in natural increase depended primarily on the economic activity of the slaves.

Higman's reconstruction of slave genealogies confirms contemporary opinion on miscegenation. It was exceptional for slave women to have children darker than themselves, and this was particularly rare when a woman's color approached whiteness. The reverse applied in the case of male slaves. A study of slave genealogies for five properties indicated that no African women had borne children of color. Higman considers it unlikely that as a rule white males forcibly violated black women; rather, he suggests, the process of miscegenation probably conformed to regular patterns, psycho-socially induced, which were known and observed by whites and slaves alike.

Using a limited sample, Higman argues that most slaves with proximate family linkages lived in nuclear units. The merit of this hypothesis would depend upon the stability of those units. Although stability is considered, Higman's argument is not wholly convincing. He is most persuasive in denying that in the nineteenth century an unprofitable slave economy was maintained merely to preserve the social status of whites. He shows that the planters organized labor with sufficient efficiency to preserve productivity levels in spite of a declining number of slaves in the most productive age group. Slavery did not collapse of its own weight in Jamaica.

Higman integrates secondary and primary materials very effectively. He has probed his data intelligently, and if some of his conclusions are guardedly offered, his caution is warranted in light of the social and environmental diversity of Jamaica. This handsomely produced book, replete with figures and tables, includes detailed appendixes and a fine bibliography.

WILLIAM A. GREEN
College of the Holy Cross

C. H. GRANT. *The Making of Modern Belize: Politics, Society and British Colonialism in Central America*. (Cambridge Commonwealth Series.) New York: Cambridge University Press. 1976. Pp. xvi, 400. \$32.50.

This excellent book, which views empire from its outermost rim, may serve as a paradigm of decolonization. Availability of evidence, assiduous research, and the small scale of the colonial society examined make possible a comprehensive study of

consequences of imperial exploitation. The contemporary history of Belize, or British Honduras, is presented in analytic detail which is almost Rankean. To read this work is to receive a remarkably clear picture of what happened in one small colony in the twentieth century. It is to learn who formed political parties, why, and with what success, as well as which economic and imperial circumstances provoked the political solutions attempted. It explains why there was no effective political agitation for self-government or decolonization before the devaluation crisis of 1950 and analyzes the continuous constitutional changes since then.

As chapters pass, one perceives the emergence of a master politician (George Price, 1950 to the 1970s) who possesses those characteristics of foresight, cunning, and persistence which characterize master practitioners in grander arenas. Here one finds the clearest and most convincing analysis yet available of the impact on Belizean politics of the still unresolved Guatemalan claim to British Honduras. The initiatives toward West Indian Federation, toward the Central American Common Market, indeed toward any kind of negotiated independence are made clear. Such independence still eludes Belize.

The author makes judgments which are debatable, even controversial, but usually makes clear the basis for his conclusions. His magisterial style can be criticized. Although analysis is vital to this book, it sometimes becomes stylistically oppressive. The author seems to be forever unlimbering social science apparatus like a howitzer battery. Academic phraseology occasionally blurs the reader's focus (for instance, see the first two paragraphs of a key chapter [p.126]). For the most part, however, the author's style is appropriately deliberate. The scholarly aids (index, endnotes, bibliography) are admirable and useful, and the work is designed for serious comparative studies.

To the historian this book offers a clear, if segmented, account of a powerless colony seeking independence in a period of general decolonization. Social scientists will find here a finely wrought critique of the plural society theory, particularly as applied in the Caribbean.

WAYNE M. CLEGERN
Colorado State University

PHILIP CARAMAN. *The Lost Paradise: The Jesuit Republic in South America*. New York: Seabury Press. 1976. Pp. 341. \$14.95.

Philip Caraman has surveyed the printed literature treating the Jesuit province of Paraguay. His extensive sources, both primary and secondary,

allow him to develop a narrative that is dramatic, compelling, and convincing.

In a straightforward manner Caraman sketches the pre-Columbian geography and ethnography of the extensive La Plata river system. He then traces the explorations and settlements of the Spanish before introducing the heroes of this volume, the missionaries of the Society of Jesus.

The difficulties involved in the pacification and incorporation of the various Amerindian peoples into missionary reductions constitute the heart of this volume. The efforts by the Jesuits from the sixteenth to the eighteenth centuries are described as constant and heroic. Chapters dealing with military, economic, artistic, and social topics are meshed into the chronological framework. Caraman develops the theme of progress and development as he traces the expansion of the "Jesuit Republic" from a Paraguayan core to adjacent areas in present-day Brazil, Argentina, Uruguay, and Bolivia. One of the author's contentions is that the Jesuits were gradually expanding and would have drawn all the Amerindians of South America into reductions if they had not been expelled by the Iberian monarchs in 1759 and 1767.

Caraman, an English Jesuit himself, is a great admirer of his hispanic predecessors, and he usually cites supporters of the order over its critics. For example, he argues that the system of reductions eventually collapsed after the Jesuits' departure not because it was too paternalistic and did not allow the natives to grow to accept lay Spanish rule, but because the successors to the Jesuits were inferior administrators lacking dedication. He points to the instructions to the lay and clerical appointees to the reductions; these "enlightened" documents mandated a continuation of the Jesuit paternalistic system.

Actually, the apologetics are of secondary importance to Caraman. Primarily he is concerned with retelling the amazing stories of discipline, dedication, and ingenuity that emerge from this chapter of colonial South American history. The professional historian will not find any new sources or interpretations. However, for the lay reader looking for historical adventures Caraman has more than adequately condensed the mass of available materials.

CHARLES J. FLEENER
Saint Louis University

RICHARD PRICE. *The Guiana Maroons: A Historical and Bibliographical Introduction*. (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1976. Pp. ix, 184. \$18.50.

This work is a valuable primer for the student of maroon societies in the Guianas, a subject and an

area still relatively unexplored by systematic scholarly investigation. The book is organized into three parts: a historical framework, a bibliographical essay, and a list of printed and mimeographed sources. The multilingual list of literature (principally in English, Dutch, French, and German) on the Guiana maroons or Bush Negroes consists of nearly 1500 items. Combined with the critical assessment in the bibliographical essay it constitutes an invaluable aid to the scholar who wishes to explore this subject. Both testify to the painstaking five years which the author devoted to the project.

In the historical survey Richard Price examines the establishment and nature of the Surinam plantation colony, the cultural content of its slave society, and the creation of the various maroon societies. The African origins of the slaves and the chronology of the establishment of runaway societies are crucial elements in the author's analysis as these go a long way in explaining the problem of African survivals. Price clearly delineates the complex evolution of the various maroon languages and societies. He emphasizes the rapid adaption of runaway slaves to their new environment and the synthesis of their cultures at an early stage of their development, and he cautions against an overreliance on the notion of African survivals.

The title of the book is misleading, as its content is almost exclusively devoted to Surinam, while the adjoining territories of French Guiana and Guyana (the former British colony) are almost completely ignored in both the text and the bibliography; an explanation of this confusing discrepancy would have been in order. In conclusion, although rationally organized, the numbering system of the bibliography could have been improved. These deficiencies, however, are easily overshadowed by the valuable contributions that this publication has to offer.

JOHANNES POSTMA
Mankato State University

WILLIAM FREDERICK SHARP. *Slavery on the Spanish Frontier: The Colombian Chocó, 1680-1810*. Norman: University of Oklahoma Press. 1976. Pp. xvi, 253. \$9.95.

There is much to praise in this study of the Colombian region of Chocó in the colonial period. It is the first real study of that frontier region in the early years when its gold and platinum production greatly increased the world's money supply. The information on slavery, race relations, and economic matters will draw historians beyond Colombian and even Latin American history.

A hot climate and torrential rains, over three hundred inches annually, made the Chocó marshy, pestilent, jungled, and uninviting except for

its gold and platinum. To gain precious metals the Spanish overcame nature and subjected both Indians and blacks to forced labor beginning in the 1670s. As a result, Indian numbers had decreased ninety percent by 1782, but they still constituted thirty-six percent of the 17,898 inhabitants, while blacks numerically had become the dominant racial group, with forty and twenty-two percent of the population as slaves and freedmen. Whites, who came to make their fortune and leave, never amounted to more than three percent of the population.

Inhospitable and walled off by mountains from the rest of Colombia, the Chocó remained a frontier region far removed from the centers of Spanish government, law, and religion. For that reason the Chocó offers scholars an interesting laboratory to test the much-debated Tannenbaum thesis that tradition, law, and the Catholic Church tempered slavery in Latin America. William F. Sharp concludes that these forces were not important in moderating the effects of slavery, but Spanish behavior toward slaves did "accord with Spanish conduct Tannenbaum . . . described" (p. 147). Slaveholders kept families together, helped them marry, and let them purchase their freedom, because such treatment insured greater productivity and profits.

Sharp's conclusions about contraband and the profitability of slavery in the Chocó are important for economic historians. He offers persuasive evidence that half the mined gold escaped government taxation and that the rate of profit was considerably higher than rates of return for slavery in the United States in the nineteenth century or for merchants and hacendados in Mexico and Colombia in the eighteenth. Since most of the profits went to slaveholders and mine-owners from outside the Chocó, a study is now needed on where the wealth flowed and in what proportions. Popayán, for example, has produced sixteen of Colombia's presidents as well as numerous archbishops, generals, and ambassadors. The Chocó supplied Popayán's elite with much of its wealth, but does that account for Popayán's disproportionate influence in Colombian society?

In summary, this is a sound treatment of a significant but previously little understood region whose importance transcends Colombian history because of its precious metals, race relations, and frontier setting.

MAURICE P. BRUNGARDT
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ROBERT G. KEITH. *Conquest and Agrarian Change: The Emergence of the Hacienda System on the Peruvian Coast*. (Harvard Historical Studies, number 93.) Cam-

bridge, Mass.: Harvard University Press. 1976. Pp. ix, 176. \$17.50.

This cleanly done volume, which deals with the early history of agrarian estates in a major region of Spanish America, was originally conceived in the tradition of François Chevalier and then modified in light of more recent trends in Latin American history. Robert G. Keith concentrates on the central Peruvian coast from the mid-sixteenth century to the early seventeenth. Even so, the scope and complexity of the topic are large.

Two chapters set the background, treating the preconquest situation and the first postconquest form of the large estate, the *encomienda*. The author gives us an intelligent and generally very adequate synthesis of previous scholarship, but I would demur on two points. First, Keith does not take into account that production, commerce, and operation within a Spanish money economy were an inseparable facet of the overall constitution of *encomiendas* from their first establishment, even before the Spaniards reached Peru. Second, he describes the pressures on the *encomienda* much more in terms of viceregal policies and disappointed candidates for *encomiendas* than in terms of the growing, ever wealthier Spanish population centered in Lima, of which the claimants were only one element, and which contained within itself the basic forces and opportunities for change in the *encomienda* system.

The following two chapters are the heart of the book; they describe the growth through the mid-sixteenth century of *chacras* or intensive, market-oriented Spanish agricultural enterprises, mostly of modest size. In a very valuable and uncontroversial way, the author pinpoints the timing of developments and shows variations from valley to valley depending on proximity of market, type of product, and local Indian precedent. In these chapters the depth of the study is considerable. Yet this is neither the exhaustive land tenure research of William Taylor or Mario Góngora, nor the massive social history of Frederick Bowser and others. Apparently Keith scanned the records only for documents directly related to land acquisition or holdings of rural estates, without attention to the total urban/rural complex or the overall personnel. One chapter on the *chacras* is entitled "The Age of the Gentleman Farmer," a phrase that would not be used by a scholar aware of the acutely marginal and plebeian status of most *chacra* operators.

A fifth chapter documents the continuing Spanish occupation of coastal land (reflecting the larger market and diminished Indian population) and the increasing consolidation of landed enterprises into the larger units that were dominant by the

seventeenth century. Here the research thins again; the author shows little vision of the manner of operation, marketing, staffing, or labor aspects of these larger units, which he calls haciendas. Still, the development itself—a major one—is well established.

JAMES LOCKHART
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ARNOLD J. BAUER. *Chilean Rural Society from the Spanish Conquest to 1930*. (Cambridge Latin American Studies, number 21.) New York: Cambridge University Press. 1975. Pp. xviii, 265. \$18.95.

Chilean Rural Society from the Spanish Conquest to 1930 is at the same time a valuable and a somewhat disappointing book. It is valuable for the original materials it makes available concerning commercial, financial, and agricultural conditions in selected Chilean regions between 1850 and 1890 as well as for its insights into the composition of Chile's political and economic elites during this period. It is disappointing, however, if we contrast the book's title and the author's stated objectives with the limited coverage actually provided.

Rather than surveying Chilean rural society from the Spanish conquest until 1930, Arnold J. Bauer's book focuses primarily on the period 1850–90. After a short chapter summarizing secondary sources on the colonial period, Bauer provides an in-depth look at certain aspects of rural Chile after 1850, with occasional comparisons to agrarian history in Europe and other non-hispanic societies. Following an overview of mid-nineteenth-century conditions in rural Chile, the author turns to considerations of the impact on Chile of an expanding European capitalism, the effects of the Californian and Australian markets on Chilean agriculture and commerce, and the role of agriculture in the Chilean economy.

The most noteworthy contribution of this book is the new perspective it provides on the workings of the system of rural credit in Chile in the years 1850–90, as well as the glimpse it permits into the relations between rural laborers and estate owners. The author's creative use of several previously unknown hacienda account books, supplemented by wills and inventories from judicial archives, gives the reader a fascinating micro-view of the nineteenth-century rural estate and of the financial lives of the landowners. Use of these intriguing sources, however, also presents some problems. From data on a handful of non-randomly selected haciendas in geographically nonrepresentative locations in Chile's central valley, the author seeks to make generalizations for the entire territory from Aconcagua to Concepción. A methodologically sounder approach would have been to

provide intensive case studies of individual haciendas or regions. Such a strategy would not have allowed sweeping generalizations, but would have had the advantage of providing carefully researched benchmarks as a basis for future scholarship.

Perhaps even more important than this methodological shortcoming is the author's serious misrepresentation of the evolution of rural class relations in Chile after 1919 in the last two chapters. To characterize rural labor in Chile in the 1920s or 1930s as a "quiet and cooperative rural mass" (p. 223) is quite misleading. To suggest that "given their free choice, most rural workers, or at least the service tenantry, would have probably supported their *patrón* against an outside candidate" ignores the reality of landlord-tenant relations in rural Chile after World War I. From 1919 to 1925 rural labor conflicts inspired by the Chilean Workers Federation (*FOCH*) so frightened landowners that they formed an "agrarian league" to combat rural labor organizations and to blacklist "troublemakers." Landowners made explicit their position that insubordination would result in eviction from the hacienda. Their agents herded rural laborers to the ballot box. In 1933 the major landowners' association (*Sociedad Nacional de Agricultura*) successfully pressured Chile's president into suspending agricultural unionization through an illegal administrative decree. Police broke strikes in the countryside. Such repression hardly suggests a "quiet and cooperative rural mass."

Despite methodological problems and errors in interpretation, however, a lucid writing style and a superb synthesis of previous studies of Chilean social development in the nineteenth century make this book a valuable resource for all students of Chilean history. It is, in places, a pioneering work that will generate controversy as well as stimulate future historical scholarship. According to the author, the book was not meant to be definitive but rather to allow "the rasher hypotheses to be challenged or clarified by the work of others." In this respect, as soon as political conditions in Chile permit, the book has every likelihood of being successful.

BRIAN LOVEMAN
San Diego State University

IAN ROXBOROUGH *et al.* *Chile: The State and Revolution*. New York: Holmes and Meier. 1977. Pp. x, 304. \$20.00.

Dedicated "to all those who fought for socialism in Chile," this book excels as the best of those which argue that President Salvador Allende's government (1970–73) failed because it did not develop a mass revolutionary party ready for armed insur-

rection: "he who makes half a revolution digs his own grave." A detailed, rigorous, indeed relentless case is made that Allende could not create socialism through a democratic bourgeois state which formed an integral part of the capitalist system. Neither dogmatic nor simplistic in their bias, the authors confront nearly all the complex questions of the Chilean road to socialism seriously and critically. Their thoughtful, usually dispassionate, analysis of the elaborate issues faced by the Chilean Left and labor achieves a high level of sophistication, clarity, and accuracy. Even scholars with less radical points of view will appreciate the abundance of information and data as well as the coherence of the controversial interpretation. But even scholars who share the authors' perspective will wish that they had provided more original research, evidence, and footnotes to support many of the assertions.

After a brief history of the labor movement and Marxist parties from the 1920s to the 1970s, this study concentrates on Allende's three years and the military junta which destroyed and succeeded him. The authors explain the rise and fall of Chilean socialism by emphasizing the country's dependent economy, militant working class, and democratic multiparty politics. Their dependency analysis is better sustained than most. However, they need improved information and definitions for Chilean social classes, particularly their economic and political ties. To contend that socialist revolution was on the agenda but betrayed by timid Leftist leaders, these British observers sometimes exaggerate working-class solidarity and revolutionary consciousness. They neglect attitude surveys, electoral results, and trade-union data which would modify their picture of worker radicalism. They also stress the proletariat rather than the more numerous and less revolutionary peasants and urban shanty-dwellers. Like the authors, Chilean revolutionaries as well as reformers were hard pressed to devise cogent ideological and strategic alternatives to the historically logical but tragic path taken by Allende. As the book shows, with more verve than subtlety, his domestic opponents, aided by the United States, proved more willing and able to overthrow the existing political system.

PAUL W. DRAKE
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Urbana-Champaign*

ARTHUR P. WHITAKER. *The United States and the Southern Cone: Argentina, Chile, and Uruguay*. Foreword by EDWIN O. REISCHAUER. (The American Foreign Policy Library.) Cambridge, Mass.: Harvard University Press. 1976. Pp. xiv, 464. \$20.00.

Arthur Whitaker's latest addition to his considerable and impressive interpretations of Latin American history not only completely recasts his *United States and Argentina* (1954), but also includes Chile and Uruguay within the basic history/foreign affairs approach of this series. Essentially the study provides three mini-histories of the distinct national states that occupy the southern tip of the western hemisphere, combined with a brief analysis of how those countries have related to the changing role of the United States in the world.

An opening chapter, "Unity and Diversity in the Southern Cone," suggests common features in geography, culture, peoples, and politics, but also recognizes the significant differences and divergences that separate the three nations. The major elements which shape those individual destinies gradually emerge within three major periods or sections defined by Whitaker: nineteenth-century foundations; development to Depression, 1910-40; and post-World War II. Two cross-national chapters once again stress common themes and experiences, one at the end of the second section and the other at the outset of the third section. Otherwise, chapters focus on individual nations and time-spans. The fourth section of the book sketches the main outlines of United States relations with each country; a review of well-known events, usually bilateral, which occurred before 1945; a challenging although brief look at the Cold War era; and an epilogue that suggests where the United States may move in the 1970s. A few statistical tables and a helpful bibliography complete the volume.

Readers will appreciate Whitaker's well-tested abilities at synthesis and analysis. There will be some who will question his acceptance of the "Southern Cone" concept, one not often used outside the field of military strategy, and at times, he does seem to have difficulty in finding comparisons or themes which run through all three countries. Others will be disappointed that, in contrast to the extended historical discussions, he has devoted only seventy-five pages to foreign policy considerations. But on balance, Whitaker has significantly expanded the coverage of Latin America within the American Foreign Policy Library and provided his public with concise and stimulating reviews of three countries that have increasingly frustrated economic analysts and political commentators with their unexpected behavior. Whitaker may not have explained that behavior, but he has certainly laid the groundwork for understanding the historical context so fundamental for current analysis.

JAMES R. SCOBIE
*University of California,
San Diego*

ADALBERTO LÓPEZ. *The Revolt of the Comuñeros, 1721-1735: A Study in the Colonial History of Paraguay*. Cambridge, Mass.: Schenkman. 1976. Pp. 214. \$12.50.

The Paraguayan uprisings of the 1720s and 1730s, best understood together as "the first of several revolts which shook the Spanish American colonial world during the course of the eighteenth century," were motivated, as Adalberto López correctly concludes, principally by the hostility of Spanish settlers in Paraguay toward the famous Jesuit mission province of the same name. Paraguayans instituted judicial proceedings which removed from office their pro-Jesuit governor in 1721, engaged in battle and defeated a Jesuit Indian army in 1724, expelled from the province another governor in 1731, and killed a third governor in 1733. Twice, in 1724 and 1732, they banished all Jesuits from the provincial capital of Asunción. Not until 1735 did colonial officialdom finally subdue the rebellious provincials, and did so then only with the aid of severe dissension in Paraguayan ranks.

López introduces his account with chapters on the traditions of political autonomy and turmoil in Paraguay, the economic, social, and strategic bases of provincial life, the evolution of Jesuit Paraguay, and the largely but not exclusively economic reasons for the mutual hostility between the settlers and the Society. He then describes the revolt itself in the next four chapters and gives some final thoughts in an epilogue. There are few factual errors. The author consulted most of the relevant secondary and primary sources with the exception of José L. Mora Merida's *Historia Social de Paraguay* and also documents in *Sección Nueva Encuadernación* in the *Archivo Nacional de Asunción*, which might help clarify his analysis of the social structure of Paraguay.

A few additional problems remain. Events in Paraguay between 1733 and 1735 are less clear than the preceding episodes, although this is probably more a reflection of anarchical conditions in the province than any fault of the author. López, however, could have more distinctly explained the relationship, if any, between the revolt in Paraguay and other rebellions in the Spanish colonies in the eighteenth century. Also, a fuller discussion of the local and imperial politics at the audiencia of Charcas, at the viceregal court in Lima, and in Madrid would have contributed to a better understanding of the relationship between events in Paraguay and larger imperial problems.

Nevertheless, this monograph's strengths far outweigh its weaknesses. It is a worthy example of narrative history. Chronologically organized, it is uncommonly clear and well written. It is now and

will likely remain the standard account of the Revolt of the Comuñeros of Paraguay.

JAMES SCHOFIELD SAEGER
Lehigh University

EZEQUIEL GALLO. *Farmers in Revolt: The Revolutions of 1893 in the Province of Santa Fe, Argentina*. (University of London Institute of Latin American Studies Monographs, number 7.) London: Athlone Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1976. Pp. 97. \$9.00.

Ezequiel Gallo uses the three agrarian revolts of 1893 in Santa Fe Province to explore issues usually treated only at the national level: the sociocultural effects of late nineteenth-century economic growth, the assimilation and politicization of immigrants, and the origins of the Radical Party.

The uprisings occurred during the tumultuous decade of the 1890s when the Baring Crisis abruptly terminated a long period of rapid economic growth, and when the newly formed Radical Party challenged the political hegemony of the entrenched Autonomista Party. Concurrently, the Autonomista government of Santa Fe levied a grain tax on local farmers and restricted municipal autonomy in the countryside, thus precipitating the agrarian revolts and inadvertently linking the Radicals with the agricultural colonists.

In his well-researched analysis of these events, Gallo first discusses the structure of agricultural colonization and then reviews the political background to the upheavals of 1893. The remaining chapters follow closely the three revolts and the subsequent elections of February 1894.

Gallo argues that two key factors—length of residency and patterns of land ownership—influenced the colonists' political stance. The settlements that led the movements originated early in the colonization process and included a high proportion of independent farmers (seventy percent in Esperanza). The other colonies, especially those in the West and South, that remained marginal to the protests, were founded later and contained a low proportion of proprietors. Less convincingly, Gallo identifies two other variables at work in the central zone: the preponderance of Swiss immigrants with strong political traditions and the cohesive effects of colonizing companies.

Unfortunately, Gallo's intriguing study suffers from some weak explanations and confusing conclusions about the causes of the revolts, their significance, and the linkages between the colonists' goals and the Radical Party's programs. In this respect, his monograph does not reflect the kind of cogent analysis exemplified by his joint effort with Silvia Sigal (1963) to correlate key variables of modernization with rural voting patterns and the

electoral successes of the Radicals. Nevertheless, Gallo firmly establishes the need for more regional studies to test comprehensive theories about Argentine history formulated from nationally focused investigations.

WILLIAM J. FLEMING
Indiana University-Purdue University,
Indianapolis

DAVID ROCK, editor. *Argentina in the Twentieth Century*. Pittsburgh: University of Pittsburgh Press. 1975. Pp. vii, 230. \$14.50.

Here is an excellent mix of essays on Argentine economic and political history, edited by Cambridge-educated David Rock, who may fairly be called a rising star among scholars studying the history of that disturbed nation.

All of the essays are valuable. More than most symposia, this one, held in 1974, pursues successfully several inter-related themes, all of which are placed within the analytical context of the attempt to unravel the causes of Argentina's destabilization.

Four of the essays deal with Anglo-Argentine economic relations and/or Perón's agricultural policies, in the period from 1880 to 1968. A. G. Ford contributes "British Investment and Argentine Economic Development, 1880-1914," centering on the theme that the pace of British investment may have been a function of a "'development cycle' within Argentina whose periodicity exceeds that of a trade cycle." In other words, the author shifts the burden of Argentina's dependence on Britain to the former, while making only the mildest concession to dependency theory in stating that the major beneficiaries of the balance of economic power, the Argentine landlords and the foreign investors, may have left "mixed legacies . . . to later generations of Argentines."

Roger Graval's essay, "Anglo-U.S. Trade Rivalry in Argentina and the D'Abernon Mission of 1929," shows that the struggle for Argentine trade was not merely a United States effort to compete with the British but was a "battle for capital assets" whose objective was to drive the British from the market.

Two provocative pieces, one by Colin Lewis on "Anglo-Argentine Trade, 1945-1965," the other by Jorge Fodor (the only Argentine and non-British contributor), "Perón's Policies for Agricultural Exports, 1946-1968: Dogmatism or Commonsense?," are especially interesting because they contradict one another. The former takes the traditional position that Perón's efforts to industrialize "at a stroke" ruined agriculture at a time when Argentina could have expanded primary exports, while Fodor, in a brilliant piece of what can only

be called pro-Perón revisionism, argues that opportunities for the expansion of agricultural production and exportation were in fact severely limited by objective foreign and domestic conditions.

Three essays—two by the editor of the volume, on "Radical Populism and the Conservative Elite, 1912-1930," and "The Survival and Restoration of Peronism," and one by Walter Little on "The Popular Origins of Peronism"—are among the most illuminating expositions of modern Argentine history I have read. Rock, as in his recent Bolton Prize-winning book, *Politics in Argentina*, first skillfully shows the failure of the Radical bourgeoisie to conquer the "buttresses of the political and economic dominance of the landed society." Then, in the final chapter, despite the hazards (and some failures) in working so close to the grain, he analyzes the economic dislocations after 1955 which prevented the re-establishment of a class alliance and finally led to the return of Perón in 1973 as a "preventive rather than a spur to revolution."

Ian Rutledge's essay on the sugar cane industry in northern Argentina in 1930-43 is of equal quality to the other papers and is loosely related to them by some fresh insight and challenges to internal dependency theory.

THOMAS F. MCGANN
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Austin

DONALD C. HODGES. *Argentina, 1943-1976: The National Revolution and Resistance*. Albuquerque: University of New Mexico Press. 1976. Pp. xii, 207. Cloth \$9.95, paper \$4.95.

Few scholars understand the complexity of the Latin American Left as well as Donald C. Hodges. His latest work offers intriguing and provocative insights into the ideology and operation of the leftist Peronist resistance to the military and civilian regimes that followed Perón's fall from power in 1955. Writing from the perspective of the Peronist and Guevarist Left, Hodges argues that militants, with Perón's blessings, constituted from beginning to end the driving force of the resistance. Parallel to this strategy of insurrection there existed a counter-strategy of conciliation pursued by conservative trade union leaders. Perón acted as moderator between the movement's two wings until his return to power in 1973. Then he turned on the Left because it had acquired a Guevarist complexion at odds with the populist philosophy of *Justicialismo*.

Hodges is at his best when discussing the bewildering ideological differences that frequently fragmented the Argentine Left. Also interesting are his accounts of regional variations in levels of union

militancy and the *cordobazo* of 1969, which apparently was not spontaneous but carefully planned by leftist students and workers. Unfortunately, Hodges' biased perspective badly distorts Peronism of the center and right. CGT leaders who opted for conciliation are cast as bourgeois traitors and/or imperialist tools who undermined the resistance in pursuit of their own selfish class interests. Labor leaders need not be cast as a species of monster, however. Since they had to negotiate with non-Peronist governments for better wages and working conditions it should come as no surprise that they became "accommodationists." The majority of rank-and-file workers obviously agreed with the policy for they remained loyal to union leadership. Indeed, Hodges' ideological blinders prevent him from seeing that the Argentine working class was and still is essentially not revolutionary.

Frankly, Hodges is not interested in portraying the Peronist Center and Right objectively, for his book, in addition to being a study of the resistance, is also a call to armed struggle. According to Hodges the leftist Peronist model has been adopted by proponents of insurrection in Chile, Bolivia, Uruguay, and Brazil. While there are some superficial similarities, I consider it far-fetched to presume that the Peronist resistance could be duplicated in other countries that lack both an organized labor movement as powerful as the Argentine and a leader as dynamic as Juan Perón. In sum, Hodges' book is both informative and biased, as one might expect in a work that is half history and half polemic.

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Recent Deaths

CHARLES CARROLL GRIFFIN, outstanding as a scholar, editor, and bibliographer of Latin American history, died on June 13, 1976. He had long been ill with emphysema. He is survived by his widow, the former Jessica Frances Jones, a daughter, Nancy Everts, and a son, Thomas Carroll.

Griffin was born in Tokyo in 1902. His father, an economics professor at Imperial University, died when he was quite young. Griffin lived in Tokyo until his grandfather, a notable missionary and educator, died in 1913. Then he came to the United States where his family lived with his uncle, Evarts B. Greene, a distinguished colonial historian. Thus he grew up in an atmosphere of scholarship. Following his graduation from Harvard University in 1922, he spent seven years in business in Argentina and Uruguay, acquiring a thorough knowledge of the language and culture. He returned to Columbia University to enter graduate school in history and teach as an instructor in Spanish. In 1931, he became Research Associate of the Library of Congress in Spain, where he enrolled in the Centro de Estudios Históricos, and supervised the transcription of historical documents in the archives of Seville and Valladolid. He received his doctorate from Columbia in 1937. Beginning in 1934, he taught at Vassar College, where he served four terms as chairman of the history department and was dean of the faculty from 1965 to 1967, the year he retired. After retirement he continued as consultant to the dean and was Executive Director of the Associated Colleges of the Mid-Hudson area. On occasion he was visiting professor at Cornell, Wisconsin, Harvard, Columbia, and Princeton, but he always returned to Vassar.

Although much of Griffin's teaching at Vassar was in United States political and diplomatic history, the main thrust of his scholarly interests embodied the Latin American field. He had wide experience and influence as a scholar in Latin America. Under the Buenos Aires Convention of 1936 for Inter-American Cultural relations (a forerunner of the Fulbright program), he was one of the first scholars to be appointed as an exchange

professor to Latin America. He taught in 1940 at the Universidad Central in Caracas, Venezuela. A Venezuelan magazine reported that far from being a typical "fat, red-faced North American," he might have been mistaken for the subject of a portrait by El Greco until one heard his "slight Anglo-Saxon accent." In subsequent years he taught at the University of Chile and lectured in Peru. He was American delegate to three meetings of the Pan American Institute of Geography and History. During World War II, he served in Washington as the assistant chief of the Division of Liaison and Research in the Office of American Republics' Affairs. He received several honors from Latin American governments and academies.

Griffin's contributions to Latin American historical scholarship were substantial. From 1949 to 1954 he was managing editor of the *Hispanic American Historical Review*. He published four books: *The United States and the Disruption of the Spanish Empire, 1810-1822* (1937), *Latin America* (1944), *The National Period in the History of the New World* (1961; with a Spanish translation in 1962), and *Los Temas Sociales y Económicos de la Época de la Independencia* (Caracas, 1961). He edited several volumes, contributed to others, and published numerous articles. His last major scholarly achievement was to edit a bibliographical volume commissioned by the Library of Congress, *Latin America: A Guide to Historical Literature* (1971). In 1970, he was the first recipient of the Distinguished Service Award of the Conference on Latin American History.

President Alan Simpson of Vassar has said of Griffin, "A dearer man we never knew—gentleman, scholar, wit He was . . . a model of good sense, good-heartedness, and fidelity. When I asked him for help he always replied that he would do anything for Vassar—and did so." He made all of these qualities, together with his rare intelligence and innovativeness and his ability to persuade adversaries to reach an amicable consensus, equally available to all who shared his enthusiasm for Latin American history. His former colleagues have established in his name a

memorial fund to create a lectureship in Latin American studies at Vassar College.

FRANK FREIDEL
Harvard University

CARROLL QUIGLEY, who taught at Georgetown University's School of Foreign Service for thirty-five years, died of a heart attack on January 3, 1977 in Washington, D.C. at the age of 66. A dynamic teacher, whose course on "Development of Civilization" was highly regarded by his students, Quigley was one of Georgetown's most respected professors. A dedicated educator, he received the student-voted Faculty Award for distinguished teaching for four consecutive years before his death and was cited by SFS alumni from 1941 to 1969 as the most influential in their undergraduate careers.

Born in Boston, Massachusetts on November 9, 1910, Quigley attended Boston Latin School and graduated magna cum laude from Harvard, where he also obtained his M.A. and Ph.D. degrees. He taught at Princeton and at Harvard before coming to Georgetown. At the School of Foreign Service, Quigley's "Dev. Civ." was a basic, required course that rarely had less than three hundred students. He was a forceful and dramatic lecturer, with intellectual vigor and wide-ranging knowledge, who placed great stress on the analytical method in his approach to the study of history. This was well illustrated in his book, *The Evolution of Civilizations* (1961), which was translated into Spanish and Portuguese. Here he sought to provide fresh insights and perspectives, in the tradition of Oswald Spengler and Arnold Toynbee, on how and why civilizations rise and fall. Quigley was convinced that the historical processes of civilization were susceptible to scientific formulations and could be logically ascertained. Although dogmatic at times in asserting his own personal opinions in the classroom, he always told his students, "Don't believe a word from anyone, including me. Go and seek out the truth yourselves."

Carroll Quigley will be remembered not only for his work on comparative civilizations, but for his challenging, monumental work, *Tragedy and Hope: A History of the World of Our Time* (1966). Twenty years in the writing, *Tragedy and Hope* was more than an account of contemporary history; it was an attempt to adopt a multidisciplinary approach in order to understand the past, using especially political science, economics, social history, cultural anthropology, psychology, and sociology. Largely a personal, interpretive account of the twentieth century, the book sought, as Quigley put it, "to look at the real situations which lie beneath the conceptual and verbal symbols." Though flawed by his own biases and prejudices, Quigley's

magnum opus is a lively, richly detailed, and informative work that amply demonstrates the strengths of his historical analytical techniques.

A long-standing member of the American Historical Association, Quigley also belonged to the American Anthropological Association, the American Economic Association, and the American Association for the Advancement of Science. He was a person of insatiable curiosity and keenness of mind, who stood apart from the more narrow, specialized scholar. Quigley frequently lectured at the Brookings Institution, the Industrial College of the Armed Forces, and the State Department Foreign Service Institute. He was a staff member of the House Committee that organized the National Aeronautics and Space Agency and a consultant to the Smithsonian Institution's Museum of History and Technology.

Shortly after his retirement, Quigley delivered a series of three lectures at Georgetown University on "Public Authority and the State in the Western Tradition: A Thousand Years of Growth, 976-1976." As a summation of his philosophy and approaches to history, it was a fitting climax to his teaching career. Peter Krogh, the Dean of the School of Foreign Service, aptly stated, "The School and Carroll were not only compatible, they were downright inseparable. And that is the way they should remain." A fund for the purpose of endowing a chair in Carroll Quigley's name has been launched, with contributions being raised among his former students. His loss is heavily felt at Georgetown.

JULES DAVIDS
Georgetown University

HOWARD ROBINSON, professor of history emeritus at Oberlin College, died in Oberlin on January 26, 1977 at the age of 91. Born in Redwood Falls, Minnesota, on July 17, 1885, he graduated from Hamline College in 1908. After taking a B.D. from Union Theological Seminary and an M.A. from Columbia in 1911, he earned the doctorate at Columbia in 1916. He taught at Wesleyan Theological College in Montreal, at Carleton College, at Miami University, and at Ohio State before going to Oberlin in 1937; he retired in 1949. He served as acting dean and dean of the College of Liberal Arts at Miami from 1930 to 1935 and as acting dean of arts and sciences at Oberlin in 1948-49.

He and his second wife, Jenny—whose death in a traffic accident occurred four months earlier—were inveterate entertainers (in both senses of the word) of undergraduates and of former students, who have long retained memories of their hospitality and of the deeper rewards that flowed from it. With the kindness and generosity went an extraordinary zest for life. Howard Robinson was a

dedicated and serious collector of books and stamps, a determined genealogist, an athlete, a hardworking farmer, an avid traveller. And every activity was touched by his superb humor, which was also a vital historical resource. It underlay the joy he took in the minutiae of history, in its curious byways and coincidences (including one in which he took considerable pride—that two of his undergraduate students became editors of this journal). But the humor also helped him to comprehend and make tolerable the confusions, cruelties, and shortcomings that make up so much of the stuff of history.

Intended for the ministry, Howard Robinson lost his faith in divinity school and thus became a historian: "I'm nominally a Presbyterian," he once told a freshman history class in reporting on a point he had looked into in Calvin's *Institutes*, "but it's a great deal worse than I thought." Working under James Harvey Robinson, he became a scholar of the Enlightenment. His doctoral dissertation was a study of reaction to the great comet of 1680; his first major monograph was *Bayle the Sceptic* (1931). Drawn naturally into English history, he published two standard textbooks, *The Development of the British Empire* (1922) and *A History*

of Great Britain (1927). In 1948 appeared *The British Post Office: A History*, no mere philatelist's study but an early milestone in the burgeoning field of administrative history; it was the first book to be awarded the Robert Livingston Schuyler Prize. In 1953 he published an abbreviated, more popular history of the post office, and from the many invitations to write postal history he chose New Zealand, his history of that postal service appearing in 1964, the same year as his book *Carrying the British Mails Overseas*. Few retirements can have equalled his in adventure and fruitfulness.

He told me once that his life began when he learned Greek. Scepticism drawn from the source was sharpened in his study of the Enlightenment. He shared with the men of the Enlightenment not only their scepticism but their universality, their immense receptiveness to experience, and an unremitting capacity for work. Fewer of his eighteenth-century forebears knew his selflessness, modesty, and balance. These virtues made him a fine scholar and one of the great teachers of his generation.

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Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

DORA B. WEINER and WILLIAM R. KEYLOR, editors. *From Parnassus: Essays in Honor of Jacques Barzun*. New York: Harper and Row. 1976. Pp. xxii, 386. \$15.00.

HENRY F. GRAFF, Presidents as Penmen. C. P. SNOW, The Classical Detective Story. DAVID CAIRNS, Spontini's Influence on Berlioz. PETER GAY, For Beckmesser. MORTIMER ADLER, Teaching and Learning. THEODORE CAPLOW, How Many Books? NELL EURICH, Learning in America. STEVEN MARCUS, Some Questions in General Education Today. ROSEMARY PARK, Liberal Education: A Chameleon. SHIRLEY HAZZARD, A Jaded Muse. C. BRIAN COX, The Editing of *Critical Quarterly*. JOHN SIMON, Translation or Adaptation? DORA B. WEINER, Three Champions of the Handicapped in Revolutionary France. ALAN B. SPITZER, Victor Cousin and the French Generation of 1820. WILLIAM R. KEYLOR, Clio on Trial: Charles Péguy as Historical Critic. FRITZ STERN, Capitalism and the Cultural Historian. CAROLYN G. HEILBRUN, Axiothea's Grief: The Disability of the Female Imagination. MORRIS PHILIPSON, Virginia Woolf's *Orlando*: Biography as a Work of Fiction. ERIC BENTLEY, From the Memoirs of Pontius Pilate: Excerpts from a New Play. RICHARD FRANKO GOLDMAN, The Great-Great-Grandnephew of Rameau. JAMES HARVEY YOUNG, Euclid + Lincoln = Kent. HUGH MACDONALD, "Un pays où tous sont musiciens . . ." WILLIAM A. OWENS, From Isaac Watts to "Heaven's Radio." DONALD A. COWAN, Science, History and the Evidence of Things Not Seen. BANESH HOFFMAN, Magic, Science and Evaluation.

RUSSELL F. WEIGLEY. *New Dimensions in Military History: An Anthology*. San Rafael, Calif.: Presidio Press. 1975. Pp. x, 419. \$14.95.

JAY LUVAAS, Military History: An Academic Historian's Point of View. THEODORE ROPP, Armed Forces and Society: Some Hypotheses. DANIEL R. BEAVER, Cultural Change, Technological Development and the Conduct of

War in the Seventeenth Century. RICHARD D. CHALLENGER, French National Security Policies, 1871-1939. ALVIN D. COOX, The Japanese Army Experience. WARREN W. HASLER, JR., High Command Problems in the American Civil War. EDWARD M. COFFMAN, American Commanders in World War I. R. H. ROY, Conscription and Voluntarism: The Canadian Experience. RICHARD A. PRESTON, The Multi-cultural and Multi-national Problems of Armed Forces. GUNTHER E. ROTHENBERG, The Army of Austria-Hungary, 1868-1918: A Case Study in a Multi-ethnic Force. WARREN B. WALSH, The Petrograd Garrison and the February Revolution of 1917. GADDIS SMITH, Multilateral Intervention in Russia, 1918-1919: Three Levels of Complexity. EARL F. ZIEMKE, Military Government: Two Approaches, Russian and American. HARRY L. COLES, The War of 1812 and the Mexican War. THOMAS E. GRIESS, A Case Study of Counterinsurgency: Kitchener and the Boers. J. BOWYER BELL, Revolts Against the Crown: The British Response to Imperial Insurgency. MAURICE MATLOFF, The Nature and Scope of Military History. HAROLD K. JOHNSON, Military Leadership and the Need for Historical Awareness.

JOACHIM RADKAU and IMANUEL GEISS, editors. *Imperialismus im 20. Jahrhundert: Gedenschrift für George W. F. Hallgarten*. Munich: C. H. Beck Verlag. 1976. Pp. 281.

IMANUEL GEISS, Die Stellung des modernen Imperialismus in der Weltgeschichte. HELMUT BLEY, Hobson's Prognosen zur Entwicklung des Imperialismus in Süd-Afrika und China. Prognosenanalyse als Beitrag zur Theorie-Diskussion. GEOFF ELEY, Social Imperialism in Germany. Reformist Synthesis or Reactionary Sleight of Hand? MASAO NISHIKAWA, Zivilisierung der Kolonien oder Kolonisierung durch Zivilisation? Die Sozialisten und die Kolonialfrage im Zeitalter des Imperialismus. ALFRED VAGTS, Die Juden im englisch-deutschen imperialistischen Konflikt vor 1914. YUKIO TOMINAGA, Anleihe und Drang nach Norden. Japanische Kriegszielpolitik im Ersten Weltkrieg. KARL HOLL, Pazifismus oder liberaler Neu-Imperialismus? Zur Rolle der Pazifisten in der Deutschen Demokratischen Partei, 1918-1930. JOACHIM RADKAU, Renovation des Imperialismus im Zeichen der "Rationalisierung." Wirtschafts-imperialistische Strategien in Deutschland von den Stinnes-Projekten bis zum

Versuch der deutsch-österreichischen Zollunion, 1922–1931.

WALTER LAQUEUR, editor. *Fascism, A Reader's Guide: Analyses, Interpretations, Bibliography*. Berkeley and Los Angeles: University of California Press. 1976. Pp. x, 478. \$20.00.

JUAN J. LINZ, Some Notes Toward a Comparative Study of Fascism in Sociological Historical Perspective. ADRIAN LYTTTELTON, Italian Fascism. WILLIAM CARR, National Socialism—Foreign Policy and Wehrmacht. HANS MOMMSEN, National Socialism—Continuity and Change. KARL DIETRICH BRACHER, The Role of Hitler: Perspectives of Interpretation. BELA VAGO, Fascism in Eastern Europe. ALISTAIR HENNESSY, Fascism and Populism in Latin America. STANLEY G. PAYNE, Fascism in Western Europe. ZEEV STERNHELL, Fascist Ideology. ALAN S. MILWARD, Fascism and the Economy. FRANCIS L. CARSTEN, Interpretations of Fascism. EUGEN WEBER, Revolution? Counterrevolution? What Revolution?

KURT VON FRITZ. *Schriften zur griechischen und römischen Verfassungsgeschichte und Verfassungstheorie*. Berlin: Walter de Gruyter. 1976. Pp. x, 622. DM 148.

Ziele, Aufgaben und Methoden der klassischen Philologie und Altertumswissenschaft. Pandora, Prometheus und der Mythos von den Weltaltern. Rezension von Entretiens I: *Der Begriff des Göttlichen von Homer bis Platon*. The Composition of Aristotle's *Constitution of Athens* and the So-called Dracontian Constitution. Rezension von J. Day and M. Chambers: *Aristotle's History of Athenian Democracy*. The Meaning of 'ektemoros'. Once more the 'ektemori'. Rezension von H. Strasburger: *Die Wesensbestimmung der Geschichte durch die antike Geschichtsschreibung*. Rezension von T. A. Sinclair: *A History of Greek Political Thought* and R. Stanka: *Die politische Philosophie des Altertums*. Rezension von A. A. T. Ehrhardt: *Politische Metaphysik von Solon bis Augustin*, Band 1 und 2. Rezension von M. Hammond: *City-State and World State in Greek and Roman Political Theory until Augustus*. 'Oper Saphestate Pistis', Thukydides I:35,5. Rezension von H. Michell: *Sparta*. Rezension von J. Humbert: *Polycratès, l'accusation de Socrate et le Gorgias*. Die Politische Tendenz in Theopomps Geschichtsschreibung. Rezension von W. R. Connor: *Theopompus and Fifth-Century Athens*. Conservative Reaction and One Man Rule in Ancient Greece. Die Bedeutung des Aristoteles für die Geschichtsschreibung. Rezension von G. Bien: *Die Grundlegung der politischen Philosophie bei Aristoteles*. Rezension von S. M. Stern: *Aristotle and the World State und Lettre d'Aristote à Alexandre sur la politique envers les cités*. The Reorganisation of the Roman Government in 366 B.C. and the So-called Licinio-Sextian Laws. *Leges sacrae et plebei scita*. Emergency Powers in the Last Centuries of the Roman Republic. Sallust und das Verhalten der römischen Nobilität zur Zeit der Kriege gegen Jugurtha (112–105 v. Chr.). The Mission of L. Caesar and L. Roscius in January 49 B.C. Pompey's Policy before and after the Outbreak of the Civil War of 49 B.C. Aufbau und Absicht des *Dialogus de Oratoribus*. Tacitus, Agricola, Domitian, and the Problem of the Principate. Totalitarismus und Demokratie im Alten Griechenland und Rom.

DEREK BAKER, editor. *The Orthodox Churches and the West*. Oxford: Basil Blackwell, for the Ecclesiastical History Society. 1976. Pp. xii, 336. £10.00.

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BERT S. HALL and DELNO C. WEST, editors. *On Pre-Modern Technology and Science: A Volume of Studies in Honor of Lynn White, Jr.* (Humana Civilitas: Sources and Studies Relating to the Middle Ages and the Renaissance, number 1. Published under the auspices of Center for Medieval and Renaissance Studies, University of California, Los Angeles.) Malibu, Calif.: Undena Publications. 1976. Pp. 233.

A. RUPERT HALL, Guido's *Texaurus*, 1335. STILLMAN DRAKE, An Agricultural Economist of the Late Renaissance. ALEXANDER G. KELLER, A Manuscript Version of Jacques Besson's *Book of Machines*, with His Unpublished Principles of Mechanics. JOSEPH NEEDHAM, China's Trebuchets, Manned and Counterweighted. THOMAS F. GLICK, Cob Walls Revisited: The Diffusion of Tabby Construction in the Western Mediterranean World. BRADFORD B. BLAINE, The Enigmatic Water-Mill. LEROY DRESBECK, Winter Climate and Society in the Northern Middle Ages: The Technological Impact. LON R. SHELBY, The "Secret" of the Medieval Masons. AMOS FUNKENKSTEIN, Aristotle, Imaginary Experiments and the Laws of Motion.

N. J. MAYHEW, editor. *Edwardian Monetary Affairs (1279–1344)*. (British Archaeological Reports, number 36.) Oxford: British Archaeological Reports. 1977. Pp. 186. £3.50.

D. M. METCALF, A Survey of Numismatic Research into the Pennies of The First Three Edwards. MAVIS MATE, Mint Officials under Edward I and II. MICHAEL PRESTWICH, Currency and the Economy of Early Fourteenth Century England. S. E. RIGOLD, Small Change in the Light of Medieval Site-Finds. N. J. PALMER and N. J. MAYHEW, Medieval Coins and Jettons from Oxford Excavations. T. H. LLOYD, Overseas Trade and the English Money Supply in the Fourteenth Century. N. J. MAYHEW and D. R. WALKER, Crockards and Pollards: Imitation and the Problem of Fineness in a Silver Coinage. J. F. HADWIN, Evidence on the Possession of "Treasure" from the Lay Subsidy Rolls. MARION M. ARCHIBALD, Wastage from Currency: Long-Cross and the Recoinage of 1279.

DALE B. J. RANDALL, editor. *Proceedings of the South-eastern Institute of Medieval and Renaissance Studies: Summer, 1974*. (Medieval and Renaissance Studies, number 6.) Durham, N.C.: Duke University Press. 1976. Pp. ix, 127. \$7.50.

DAVID HERLIHY, The Medieval Marriage Market. SIEGFRIED WENZEL, Vices, Virtues, and Popular Preaching. HANS J. HILLERBRAND, The Popular Dimension of the Reformation: An Essay in Methodology and Historiography. GILES E. DAWSON, Problems in Editing Sixteenth- and Seventeenth-Century Letters. JUAN BAUTISTA AVAL-LEARGE, Vital and Artistic Structures in the Life of Don Quixote.

Folie et déraison à la Renaissance: Colloque international tenu en novembre 1973 sous les auspices de la Fédération Internationale des Instituts et Sociétés pour l'Étude de la Renaissance. (Travaux de l'Institut pour l'Étude de la Renaissance et de l'Humanisme, number 5.) Brussels: Éditions de l'Université de Bruxelles. 1976. Pp. 234. 525 FB.

MAURIZIO BONICATTI, Le concept de la déraison dans la tradition de la culture musicale non religieuse à l'époque de l'Humanisme. PAUL FORIERS, La condition des insensés à la Renaissance. R. H. MARIJNISSEN, Bosch and Bruegel on Human Folly. ANDRÉ STEGMANN, Sur quelques aspects des fous en titre d'office dans la France du XVI^e siècles. HYACINTHE BRABANT, Les traitements burlesques de la folie aux XVI^e et XVII^e siècles. TILDE SANKOVITCH, Folly and Society in the Comic-Theatre of the Pleiade. ENRICO CASTELLI-GATTINARA, Quelques considérations sur le Niemand et . . . Personne. ALOÏS GERLO, *Badius Ascensius* "Stultiferae Naves" (1501), a Latin Addendum to Sebastian Brant's "Narrenschiff" (1494) (Ship of Fools). JEAN CÉARD, Folie et démonologie au XVI^e siècle. MARCEL DE GRÈVE, Le discours rabelaisien, ou la raison en folie. WILLIAM MELCZER, Did Don Quixote Die of Melancholy? CARLO OSSOLA, Métaphore et inventaire de la folie dans la littérature italienne du XVI^e siècle. HENRI PLARD, Folie, subversion, hérésie: La polémique de Thomas Murner contre Luther. SYDNEY ANGLO, Melancholia and Witchcraft: The Debate between Wier, Bodin, and Scott.

RAGNHILD HATTON, editor. *Louis XIV and Absolutism*. Columbus: Ohio State University Press. 1976. Pp. xiii, 306. \$17.50.

E. H. KOSSMANN, The Singularity of Absolutism. G. DURAND, What is Absolutism? R. MOUSNIER, The Development of Monarchical Institutions and Society in France. F. DUMONT, French Kingship and Absolute Monarchy in the Seventeenth Century. F. BLUCHE, The Social Origins of the Secretaries of State under Louis XIV, 1661-1715. A. LOSSKY, The Intellectual Development of Louis XIV from 1661 to 1715. J. LEVRON, Louis XIV's Courtiers. J. ORCIBAL, Louis XIV and the Edict of Nantes. G. LIVET, Royal Administration in a Frontier Province: the Intendancy of Alsace under Louis XIV. J. MEUVRET, Fiscalism and Public Opinion under Louis XIV. P. DEVON, Manufacturing Industries in Seventeenth-Century France. ARMEL DE WISMES, The French Navy under Louis XIV. J. BOUVIER, The Protestant Bankers in France, from the Revocation of the Edict of Nantes to the Revolution.

RAGNHILD HATTON, editor. *Louis XIV and Europe*. Columbus: Ohio State University Press. 1976. Pp. xiii, 311. \$17.50.

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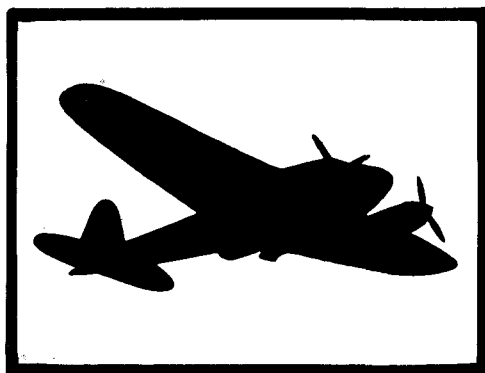
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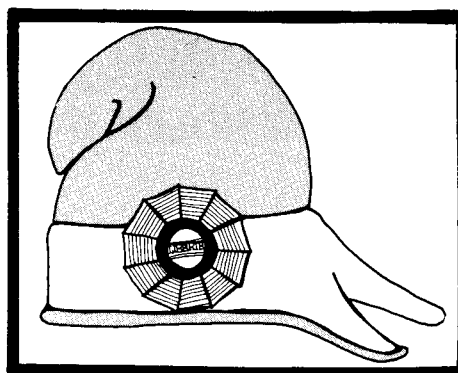
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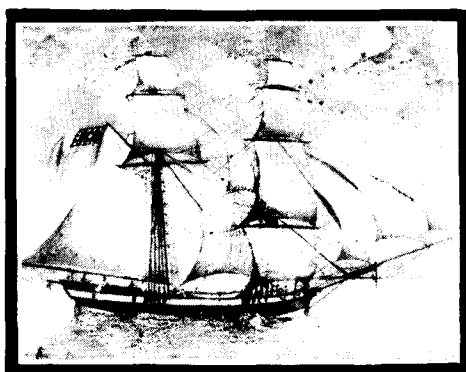
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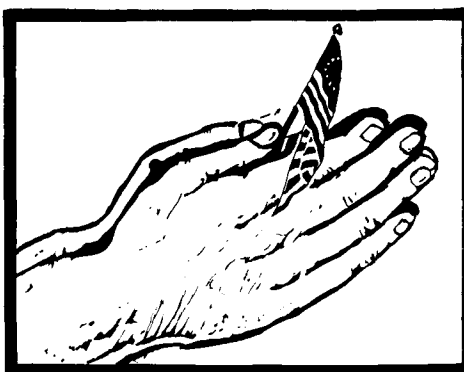
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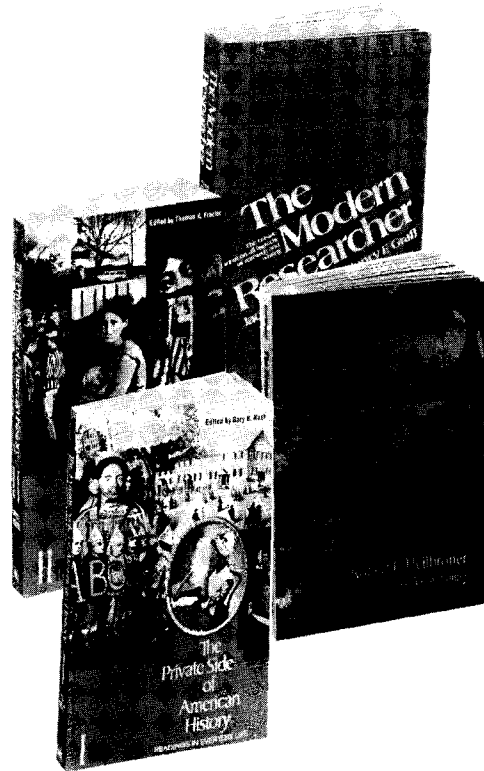
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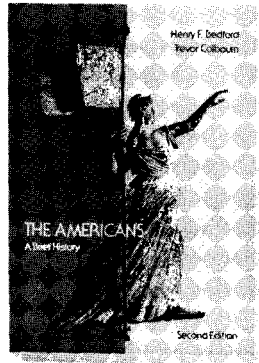


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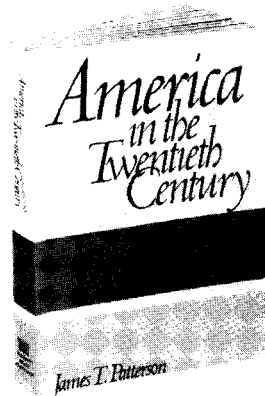
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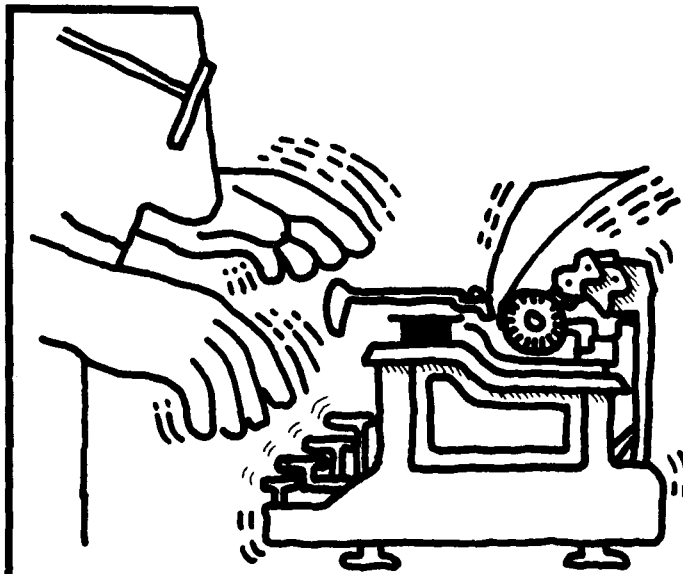
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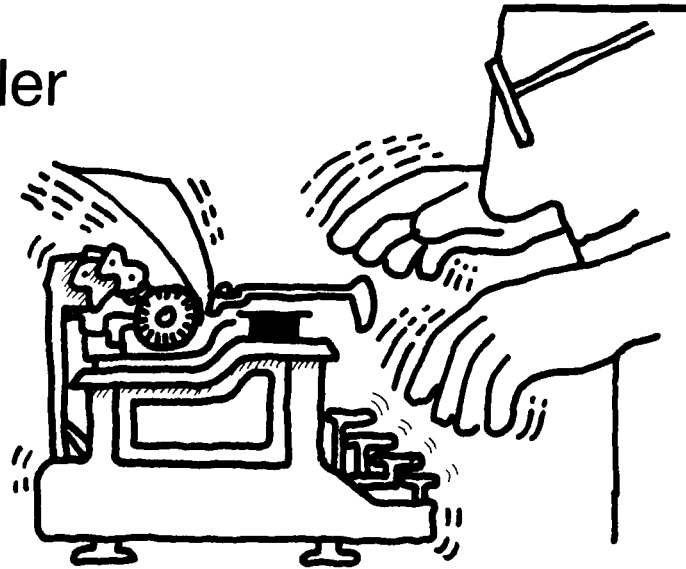
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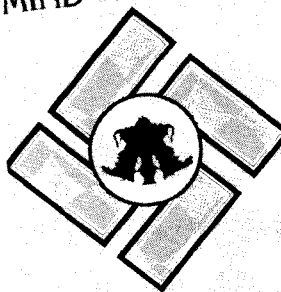
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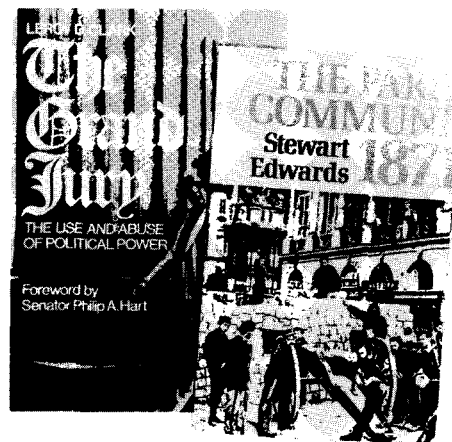
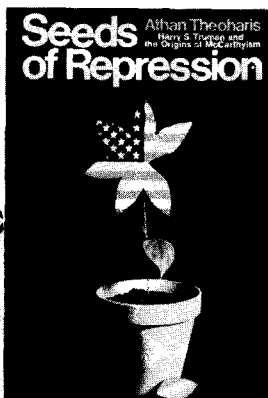
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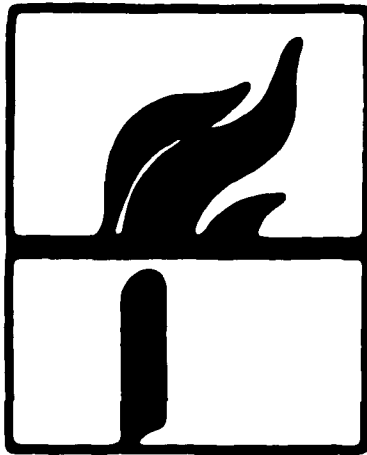
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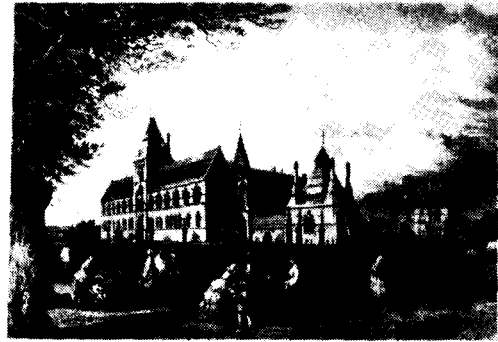


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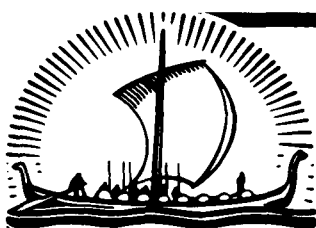
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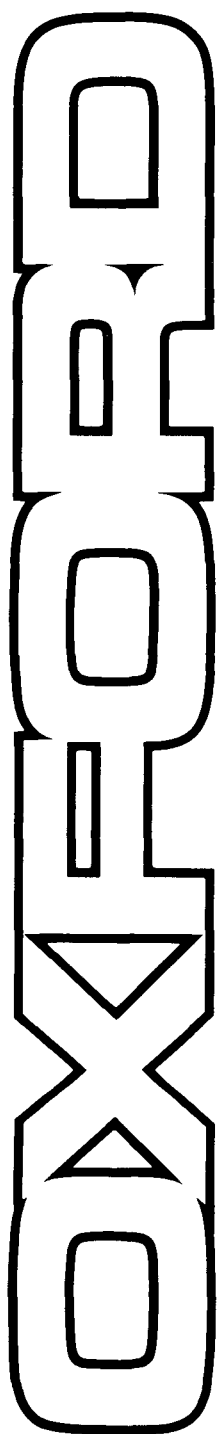
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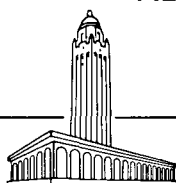


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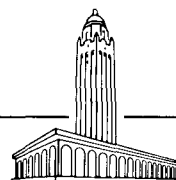
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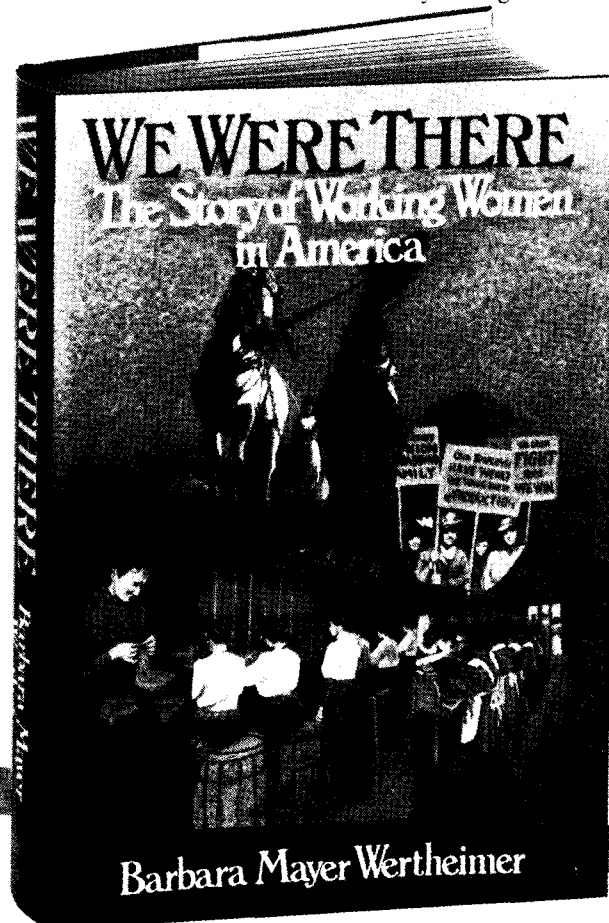
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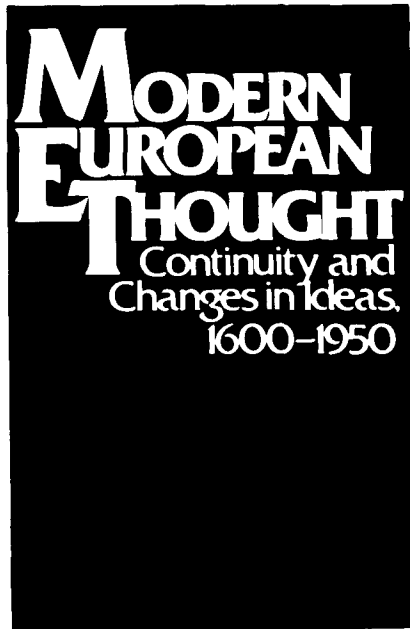
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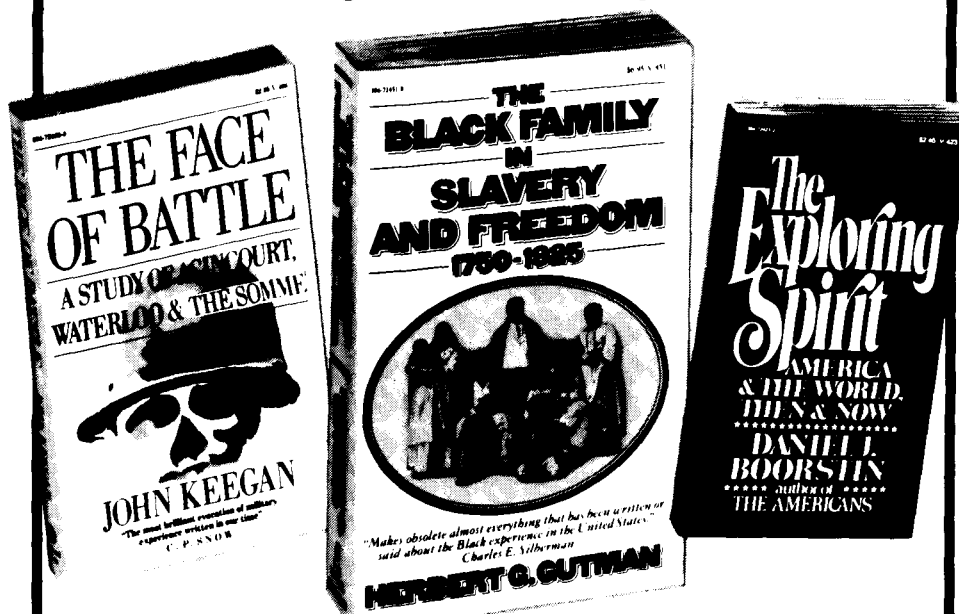


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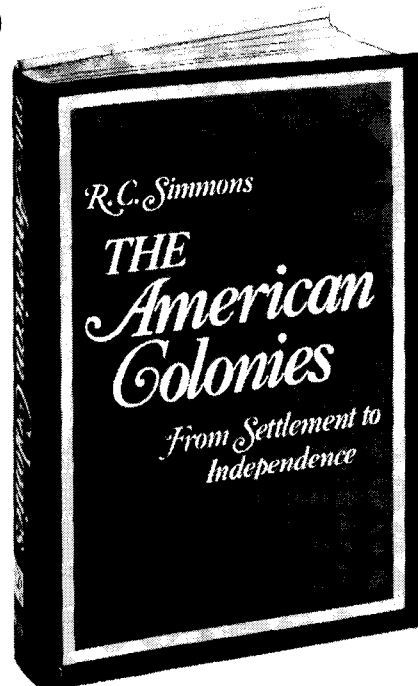
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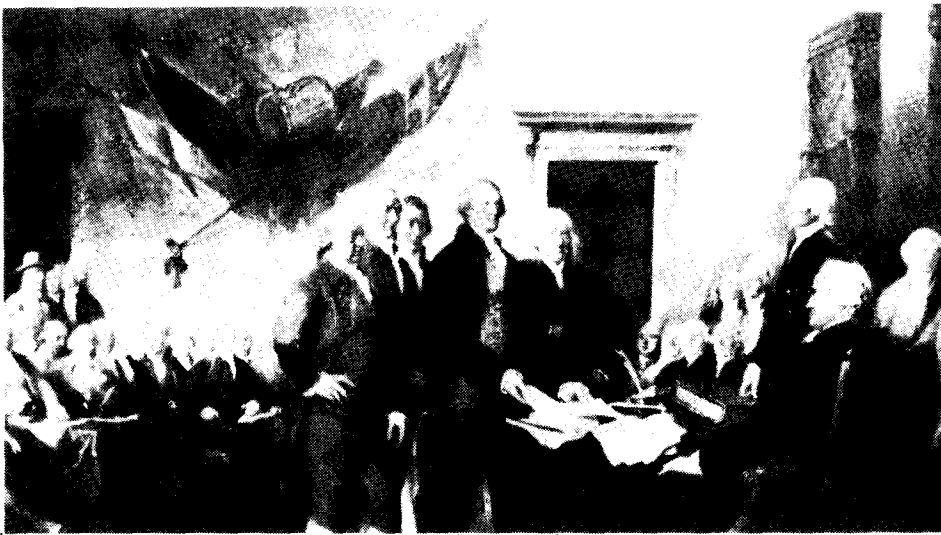
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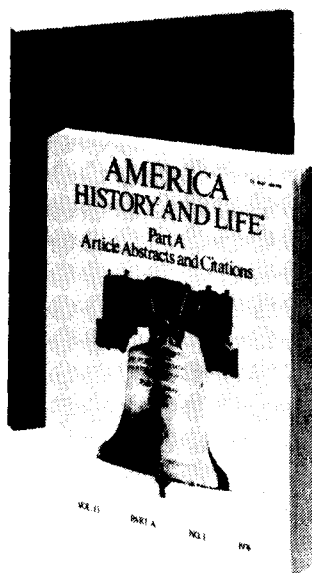
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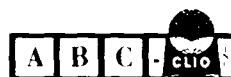
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